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THE INFINITIES AROUND US.

By PASTEUR.

"WHAT is there beyond this starry vault? More starry skies. Well, and beyond that? The human mind, driven by an invincible force, will never cease asking, What is there beyond? . . . . It is useless to answer 'Beyond are unlimited spaces, times, or magnitudes.' Nobody understands these words. He who proclaims the existence of an Infinite—and nobody can evade it—asserts more of the supernatural in that affirmation than exists in all the miracles of all religions; for the notion of the Infinite has the twofold character of being irresistible and incomprehensible. When this notion seizes on the mind, there is nothing left but to bend the knee. In that anxious moment all the springs of intellectual life threaten to snap, and one feels near being seized by the sublime madness of Pascal. Positivism unceremoniously thrusts aside this positive and primordial notion, with all its bearings on the life of human societies. Everywhere I see the inevitable expression of the Infinite in the world. By it the supernatural is seen in the depths of every heart. The idea of God is a form of the idea of the Infinite. As long as the mystery of the Infinite weighs on the human mind, temples will be raised to the worship of the Infinite, whether the God be called Brahma, Allah, or Jehovah; and on the floor of those temples you will see kneeling men absorbed in the idea of the Infinite. Metaphysics do but translate within us the paramount notion of the Infinite. The faculty which in the presence of beauty leads us to conceive of a superior beauty—is not that, too, the conception of a never-realised ideal? What are science and the passion for comprehending anything else, then, but the effect of the stimulus exercised upon our mind by the mystery of the universe? Where is the real fountain of man's liberty? where the true source of woman's dignity, but in the conception of the Infinite, in presence of which all men are equal?"

THE FUTURE OF KNOWLEDGE.

M. PASTEUR has chosen the occasion of his reception in the Academy to speculation largely upon what is in the nature of the case undemonstrable. But the truth is that the career of a great scientific discoverer suggests speculations as little demonstrable, perhaps, but of a different kind from these. Ever since thought began, mankind has wondered as to its own nature and its own destiny. It will go on wondering to the end of time, whatever new facts science may bring to light, whatever new worlds beyond the Milky Way or within the compass of a speck of dust may be revealed by telescope or microscope. It may be allowed, however, in the presence of a personality like that of M. Pasteur, or of the still greater discoverer whose loss the world is mourning, to look forward upon the future of knowledge, to ask how far all these new acquisitions will in the future modify our life, our practice, our methods of study. M. Renan, to whom a curious fortune gave the task of receiving M. Pasteur, has, in an interesting passage of his own autobiography, given it as his belief, that a century hence mankind will study very little else than physical science. The time, he thinks, will come when the historical sciences will be thrust into the background; all that they have to teach will be known, and men will feel comparatively little interest in their own past. On the other hand, the more they know of nature the more there will be to be known. Chemistry and physiology offer inexhaustible fields for research; and the truths which they reveal will prove more and more interesting to mankind. It is very difficult to say what men will think or do a hundred years hence; but it seems likely enough that this will be the tendency of study. Certainly, even now, the men of science are becoming more and more important factors in the life of all of us. They are little by little winning the fight against disease; they are giving us facts, and enabling us to found our beliefs on the sure ground of knowledge. Their influence must surely become greater and greater as time goes on; for humanity always reserves its highest honours for those who teach it to know.—*Times.*

THE APPROACHING ECLIPSE.

NOTE.—Some readers of KNOWLEDGE have been perplexed by my statement that the Editor of KNOWLEDGE had appointed Mr. Proctor Special Correspondent in Egypt, but that it was not certain whether he could go. I had no wish to mystify any of my readers. Perhaps the following statement will make all clear:—

Mr. R. A. Proctor, student of science, has been obliged, after careful consideration, to decline to accede to the earnest wish of Mr. Proctor, Editor of KNOWLEDGE, that he should go to Egypt to view the eclipse. It did not seem desirable that while KNOWLEDGE is still so young it should be left to run alone so long. Certainly, not less than six weeks would have been required for the proposed journey.

THE path of the centre of the moon's shadow across Upper Egypt during the eclipse of May 17 is shown in the accompanying map. The following table is given by Mr. Hind, superintendent of the *Nautical Almanac*, in *Nature* (Mr. Hind speaks of the eclipse of May 16, having probably become so accustomed to astronomical time that, in his mind, the morning hours of May 17, up to noon, belong to May 16; half-past eight in the morning of May 17, in common parlance, is with him half-past twenty,

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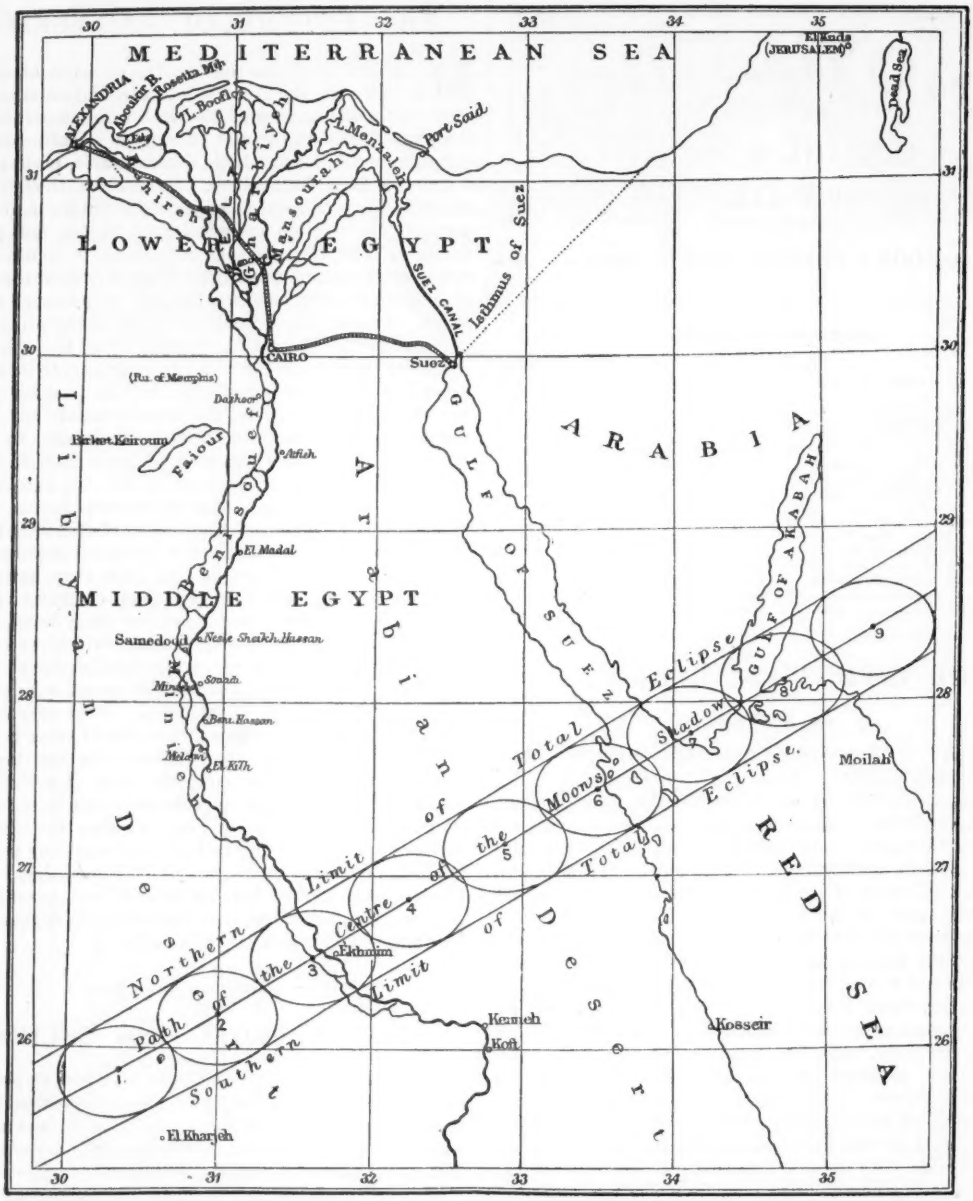


May 16; but probably most of our readers are more familiar with half-past eight than with half-past twenty).

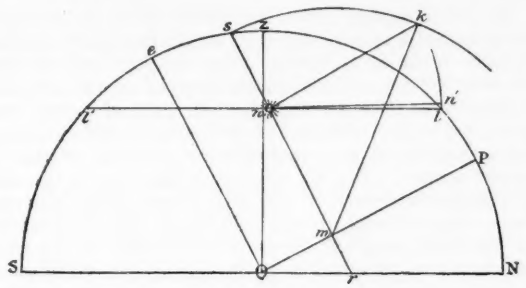
Greenwich mean time, May 16.	Longitude E.	Latitude N.	Duration of totality.
h. m. s.	deg. m.	deg. m.	m. s.
18 22 30 ...	30 18·2 ...	25 50·9 ...	1 9·2
18 23 45 ...	30 58·0 ...	26 11·6 ...	1 10·6
18 25 0 ...	31 37·0 ...	26 31·9 ...	1 12·0
18 26 15 ...	32 15·3 ...	26 51·8 ...	1 13·3
18 27 30 ...	32 52·9 ...	27 11·2 ...	1 14·6
18 28 45 ...	33 29·9 ...	27 30·2 ...	1 15·9
18 30 0 ...	34 6·1 ...	27 48·9 ...	1 17·1
18 31 15 ...	34 41·9 ...	28 7·2 ...	1 18·3
18 32 30 ...	35 17·2 ...	28 25·2 ...	1 19·5

Thus in longitude 31° 37′ E., lotitude 26° 32′ N., a point close upon the Nile, the duration of the total phase is 1m. 12s.. and the middle at 20h. 31m. 28s. local mean time. The central line crosses the Nile about a degree north of Luxor, one of the stations occupied for the observation of the last transit of Venus.

The eclipses in the map represent the outline of the moon's shadow (the umbra or total shadow), at the successive epochs mentioned in the above table. Some readers may, perhaps, be interested to learn how the shape and position of the shadow (its size being determined by the calculated duration) have been deduced from a simple process of construction, which also gives other useful information.

Draw the semicircle SZN, to represent the sky at Station 3 (where most of the observers will be) at the moment of central eclipse, N being north point, S the south, the east and west points at O, and Z the zenith, or point vertically overhead, so that OZ is perpendicular

to SN. Take arc NP of 26° 51′·8, giving P the pole, and draw Oe perpendicular to OP, representing the equator. Take *es* equal to the sun's northerly declination at the time of eclipse, about 19° 20′: then *smr*, perpendicular to OP.



represents the sun's diurnal course for that declination. Open out this course by supposing it turned round radius *sm* into circle *sk*. Take *sk*, corresponding to arc of sun's course, from moment of eclipse to solar noon at Station 3. This, correcting 8 h. 31 m. 28 s. by 3 m. 51 s. (to be added to local meantime), so that apparent time is 8 h. 35 m. 19 s., corresponds to 3 h. 24 m. 41 s., or, in arc measurement, to 51° 10′ 15″ (which we take off as angle *smk* with a protractor. Then rotate *sk* back again, carrying *k* along the perpendicular *k* ⊙ to the sun's true place ⊙ on the sky of Station 3 at the moment of central eclipse. Obviously, then *l'n* ⊙ *l* parallel to SON gives *l*N, the sun's altitude at the time of central eclipse, and the construction makes this 43°, which is within a minute or two of arc of the true altitude.

The elliptic shadow of the moon has, therefore, its major axis exceeding its minor axis in the same degree that OS exceeds O*n*. (If we want this ratio exactly, we turn to a table of natural sines for sine 43°, giving 682 : 1000.)

To determine the direction of the shadow's longer axis, we note that ⊙ is very nearly due east, but a little north of it. An arc *n'* about *n* as centre, and *n*, (more exactly a perpendicular from *n'* to *n*, but for so small an angle the arc does as well) taken equal to *n* ⊙ gives *n'n*, the small angle—about 1½ degrees—by which the sun will be north of east. Thus the larger axis of the elliptic shadow lies nearly east and west, but its eastern end a little north, so that the axis is inclined about 1½ degrees to the latitude parallel through Station 3. The size of the shadow is determined by the consideration that with the determined shape and position the shadow must cover as much of the line of central eclipse as corresponds to the motion of the shadow's centre in 1m. 12s.

Similarly for the other shadows, and of course, lines touching all these ellipses (i) above and (ii) below, give (i) the northern and (ii) the southern limits of total eclipse.

DR. SIEMENS ON SOLAR ENERGY.*

BY THE EDITOR.

IN this theory there is suggested a fan-like action, by which hydrogen, hydro-carbons, and oxygen are supposed to be drawn in enormous quantities towards the polar surface of the sun. During their approach they are supposed to pass from their condition of extreme attenuation and extreme cold, to that of compression, accompanied with rise of temperature, until on approaching the photosphere they burst into flame, giving rise to great development of heat, and a temperature commensurate with their point of dissociation at the solar density. The result of their combustion is aqueous vapour and carbonic acid or carbonic oxide, according to the sufficiency or insufficiency of oxygen present to complete the combustion, and these products of combustion in yielding to the influence of centrifugal force will flow towards the solar equator. . . . *So much* we may regard as possible, though much would have to be proved before it could be regarded as probable. But Dr. Siemens goes on to say that the matter thus carried towards the solar equator *will be thence projected into space.*

Now there can be nothing simpler than the considerations on which such projection into space would depend. The question whether a body moving in a particular way at any part of the sun's surface will travel outwards into space, or will not travel outwards, can be answered according to certain very definite laws. If the velocity of its motion exceeds a certain amount, the body will recede from the sun; if it falls short of that amount the body will tend to approach the sun's centre; if the body has just that velocity, then the body will neither recede nor approach. Now it suggests the idea of tremendous centrifugal tendency to say that at the sun's equator the velocity is 4·41 times the tangential velocity (at the equator) of our earth. Bodies do not fly from our earth's equator on account of the enormous tangential velocity there (more than a thousand miles per hour); but it is easy to imagine, as Dr. Siemens evidently does, that with the much greater velocity at the sun's equator there may be such a tendency as his theory requires. What is, however, the actual state of the case? Centrifugal tendency varies in the first place as to the square of the velocity; and squaring 4·41, we get 19·45; so that if our earth were to rotate 4·41 times as fast as she actually does, the centrifugal force at the equator would be increased 19·45 times. Even that would not be nearly enough to make bodies fly off at the equator. (In fact, it can easily be shown that for bodies just to become weightless at the equator the earth should rotate in 1⅓ hours, or *sixteen* times as fast as at present.) But this is only a small part of the matter. Centrifugal force not only varies as the square of the velocity, but inversely as the distance from the centre of motion. So that, as the sun's diameter exceeds the earth's about 108 times, centrifugal tendency at his equator is diminished in this degree, so far as this particular circumstance is concerned. Increasing the tendency 19·45 times and reducing it 108 times, means in all reducing it to about two-elevenths of the centrifugal tendency at the earth's equator. Yet even this is not all. Not only is the centrifugal tendency at the sun's equator less than a fifth that at the earth's equator, which diminishes by a very small part the force of terrestrial gravity, but the centrifugal tendency due to the sun's attractive force is very much greater at the sun's surface than terrestrial gravity at the earth's equator. It is roughly about twenty-seven times as great. Thus the centripetal tendency of matter at the sun's equator is very much greater (many hundreds of times greater) than its centrifugal tendency; and there is not the slightest possibility of matter being projected into space from the sun's surface by centrifugal tendency. Nor is there any part of the sun's mass where the centrifugal tendency is greater than at the surface near the equator. So that, whatever else the sun may be doing

* In No. 20, for March 17, there is an admirable *résumé*, by Dr. Carpenter, of Dr. Siemens' theory of the Conservation of Solar Energy. The theory appears to us unsound as respects both its chief requirements. We now give the reasoning which proves, we believe, first, that the solar energy could not be utilised in the way suggested; and, secondly, that, as a matter of observed fact, it is not so utilised.—ED.

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to utilise his mighty energies, he is certainly not throwing off matter constantly from his equatorial regions, as Dr. Siemens' theory requires.

This being so, the theory failing thus in a matter absolutely essential to its validity, we may feel less tempted than perhaps we otherwise might be to endeavour to overlook other difficulties, though these on careful consideration appear scarcely less decisive. It might perhaps appear a work of supererogation to consider difficulties when we have already noted an impossibility. But some perhaps will consider that, although the sun may not, after drawing to himself the matter occupying space, reject it from him in the manner supposed, he may reject it in some other manner. If so, there might still be reason for inquiring how far it is likely that the sun's rays may be utilised when falling on the matter occupying space, in the way suggested by Dr. Siemens. His idea is that solar radiation acting on the aqueous vapour and carbonic acid gas, and other compound gases supposed to occupy interplanetary and interstellar space, may dissociate such compounds, and that solar energy may thus be utilised, instead of being wasted.

Now, if the rays of heat (and light) are thus utilised within the solar domain, regarding that if we please as extending many times further than the orbit of Neptune, they have either done their work and have been completely utilised, or they have not. If they have done their work, these rays proceed no further, and the sun would therefore be invisible from any point outside his own domain. (For we must not fall into the mistake of supposing that light and heat can be considered separately in this inquiry: those solar rays which give us what we call light, give us also a large quantity of the solar heat, and the mystery of seemingly infinite waste would remain, even if we supposed that only those heat rays which are not also light rays were utilised in the way supposed. Apart from this, Dr. Siemens specially shows how the light rays act in accordance with his views.) Now, what is true of our sun is true of other suns, the stars. They also ought to be invisible outside their several domains, but as a matter of fact they are visible. If, on the other hand, the solar rays have *not* done their work in traversing what may be regarded as the solar domain, the mystery of infinite waste is not removed, scarcely even diminished, by Dr. Siemens' theory. If those other suns, the stars, are able to send across the vast distances which separate us from them, such supplies of light (to say nothing of stellar heat, which Huggins and others have measured) that by measuring it we can say that all of them are suns like our own, but many far larger and giving out much more light than he—what is the amount of work which we can suppose the stellar rays to have done on their way? If they have done much (in proportion to the total quantity which they are capable of doing), then the stars must be very much larger, brighter, and hotter than we suppose them to be, and already we regard them as the rivals, and something more than the rivals, of our sun. If they have done little, the mystery of infinite waste remains.

In the case of the Siemens' regenerative furnace, we know that the heat is utilised in the particular manner intended, not only because we find the heat so saved doing its proper work, but because we find that this heat no longer goes idly up the furnace chimney, as before. The heat cannot be doing its full work in the furnace if part goes up the furnace chimney; but also, part cannot be going up the furnace chimney if the heat is doing its full work. This, however, is what Dr. Siemens' theory requires the solar heat to do. It is to be continually utilised in dissociating compound vapours in interplanetary space, although it is continually passing beyond interplanetary space to shine through interstellar space, and to show our sun as a star to worlds circling round his fellow stars the suns. We have, in fact, the fallacy of the perpetual motion in a modified form.

We are compelled, then, regretfully perhaps, but still unhesitatingly, to give up that satisfaction which, as Dr. Siemens remarks, we should gain, could we believe that our solar system need "no longer impress us with the idea of prodigious waste through the dissipation of energy into space, but rather with that of well-ordered, self-sustaining action, capable of perpetuating solar radiation to the remotest future." Yet though not in this way, to this end all thoughtful study of the mechanism of the universe seems unquestionably to tend; not by centrifugal tendencies of the kind imagined, for none such exist; not by work which, viewed in reference to the universe as we know it, means endless production without exhaustion; but in other ways (associating perhaps our visible universe with others, permeating it as the ether of space permeates the densest solids, and in turn with others so permeated by it) there may be that constant interchange, that perpetual harmony, of which Goethe sung—

> See all things with each other blending,
> Each to all its being lending,
> Each on all in turn depending:
> Heavenly ministers descending,
> And again to Heaven uptending,
> Floating, mingling, interweaving,
> Rising, sinking, and receiving—
> Each from each, while each is giving
> On to each, and each relieving
> Each—the pails of gold. The living
> Current through the air is heaving;
> Breathing blessings see them bending,
> Balanced worlds from change defending,
> While everywhere diffused is harmony unending.
>
> From the *Cornhill Magazine*.

Since this article appeared in the *Cornhill Magazine*, Dr. Siemens has called my attention to a letter of his in *Nature*, in which he answers the objection relating to the centrifugal force. Next week I shall endeavour to find space for his reasoning *in extenso*; but here I must content myself by noting that it amounts simply to this: That taking two equal portions of gaseous matter at equal density and temperature, and equi-distant from the sun's centre, one at the sun's pole, the other at the equator (sharing in the sun's motion of rotation), the former is drawn with greater force towards the centre of the sun than the latter,—and that, therefore, a polar inflow and an equatorial outflow must take place, provided only that space is not empty, as supposed by Laplace, but filled with either an elastic or non-elastic fluid. This reasoning shows undoubtedly that under the imagined conditions there would not be equilibrium, and therefore those conditions would not exist. Motion would take place until equilibrium was obtained. But no one familiar with the mathematics of hydrodynamics will, on consideration of the matter, *maintain* (though, by a passing forgetfulness, he might *assert*) that, even if the impossible conditions suggested by Dr. Siemens could exist for a moment, the absence of equilibrium would lead to continuous motion outwards in the sun's equatorial plane. The surfaces of equal pressure would pass from the spherical to the spheroidal form, and would for a time oscillate on either side of the form they would finally assume; but there would be no continuous motion either of inflow or of outflow. I may note, further, that Dr. Siemens' view respecting what Mairan supposed, and Laplace disproved, is not correct. His comparison also between the loss of

solar energy of rotation, due to the fan-like action he attributes to the sun, and that resulting from the tides on the earth, is unsound. However, I must defer to next week any further comments on this subject.

THE ANTIQUITY OF MAN IN WESTERN EUROPE.

BY EDWARD CLODD.

PART II.

THE division of Palæolithic time suggested by M. de Mortillet is as follows:—*

Eolithic.....	Thenaisian...	Stone split by fire.	
Palæolithic.	Acheulian ...	Age of the	Mammoth.
	Mousterian...	,,	Cave Bear.
	Solutrian ...	,,	Reindeer and Mammoth.
	Magdalenian	,,	Reindeer.
			The Cave period.

The evidence in support of the presence of man in Europe in mid-Tertiary times consists of worked flints found in the *calcaire de Beauce*, a Miocene stratum at Thenay, in Loire-et-her, hence the term Thenaisian. The symmetrical form of the flakes; the "bulb of percussion," as it is called, *i.e.*, the conical projection at the end of a flint where the blow striking off a splinter is given; the rough chipping round the edges and the traces of wear and tear, are cited by their discoverer, the Abbé Bourgeois, as proofs of human origin. Moreover, he believes that they were fractured by the aid of fire, or used as "pot-boilers,"† which would be conclusive evidence, if proved. But it is not placed beyond doubt that the flints may not have come from previously-disturbed and later deposits nearer the surface, which is strewn with stone implements, and although similar finds are recorded from the Miocene beds of the Tagus, and bones with apparently designed scratches and notches have been found in the faluns of Pouancé, the attitude of most anthropologists is to wait for additional evidence. "*Ex pede Herculem*," says the adage, "you judge Hercules by his foot," but even the foot of Tertiary man "comest in such a questionable shape" that we cannot take his measure from it. Indeed, as the foregoing table shows, M. de Mortillet passes without pause from the relics of the Thenay beds to those of St. Acheul, which are within the Post-Pliocene, Pleistocene, or Quaternary period, as it is variously called, and which are now admitted as conclusive regarding man's presence in Western Europe by every anthropologist of repute.

But before describing these in such detail as the importance of the matter demands, let us glance at the momentous changes in Europe which appear to have preceded the arrival of Palæolithic man. These may, in measure, account for the scantiness of material yet producible, and for the gaps in the sequence of geological monuments bearing on the past history of man. "If we consider," Sir Chas. Lyell remarks,‡ "the absence or extreme scarcity of human bones and works of art in all strata, whether marine or fresh water, even in those formed in the immediate proximity of land inhabited by millions of human beings, we shall be prepared for the general dearth of human memorials in glacial formations, whether recent, pleistocene, or of more ancient date. If there were a few wanderers over lands covered with glaciers, or over seas infested with icebergs, and if a few of them left their bones or weapons in moraines or in marine drifts, the chances, after the lapse of thousands of years, of a geologist meeting with one of them must be infinitesimally small."

At the close of the Pliocene Age, the land area was greatly enlarged by slow elevation. The German Ocean, which during that period had covered East Anglia, was "high and dry." As evidenced by the forest-beds traced from Cromer to Kessingland, oaks, firs, yews, birches, and smaller trees abounded; alders flourished in the congenial swampy land; water-lilies blossomed on the rivers, from whose deposits the smacksmen on the Dogger Bank dredge up to-day vast numbers of bones of mammals then wallowing in the slime and roaming through the jungles—huge elephants, rhinoceroses, hippopotamuses, cave bears, wolves, (co-temporaries, be it remembered, of man), and even "several large estuarine and marine mammalia, such as the walrus, the narwhal, and the whale."* But, as testified by strata superposed upon the Norfolk forest beds, the temperature gradually declined, until an arctic cold prevailed; the land once more sank beneath the "azure main," and the long, though intermittent, reign of the Ice Age set in. The effects of this in the rounded hills of our island, the *roches moutonnées* of the continent (so called from their resemblance to sheep lying down); in the striated or grooved and polished rock-surfaces; in the erratic blocks—"foundlings," as the Swiss happily name them—deposited in districts far from their parent rocks, as, for example, the occurrence of Scandinavian boulders on the plains of Saxony; in the mounds of sand and gravel, and the deposits of "till" or clay crammed with stones of all sorts and sizes and scantily charged with derived and broken fossils, were long the puzzle and problem of geology, and the source of numberless legends. They were referred to every cause except the true one, until Agassiz, after long study of glacial action in Switzerland, proved them to be due to the mechanical effects of ice. What brought about such alterations of climate as to swathe the northern hemisphere in a vast ice-sheet at one period, and to clothe it within a few degrees of the pole with the vegetation of temperate climes in another period, is explained to the satisfaction of most competent judges by Dr. Croll's theory.† That is to say, at certain periods, irregular in their recurrence, the earth's orbit becomes much more elliptical, and its distance from the sun correspondingly greater. If, when this period of greatest ellipticity happens, the incidence of the seasons has been changed by the precession of the equinoxes,‡ the summer would be too brief to undo the work of the long winter, and ever-increasing accumulations of snow and ice would result. In the course of thousands of years these conditions would be reversed, and the climates of northern and southern hemispheres change places.

Such is, in brief, the explanation of those remarkable conditions which either beset or immediately preceded palæolithic man, and, only staying to remark that, with subsequent upheaval of the land, Britain was once more joined to the Continent, we may pass without further break of story to the sure ground where his "works follow him."

More than fifty years ago, many of the bone-caves of

* "Cf. Matériaux pour l'Histoire de l'Homme." Second Series. Vol. II., p. 545.

† In the absence of earthen or metal pots, we find the practice of dropping red-hot stones into vessels of skin, wood, or bark, widespread among ancient and modern savages.

‡ "Antiq. of Man," 4th ed., p. 246.

* Lyell: "Antiquity of Man," p. 258.

† "On the Physical Cause of Change of Climate during Geological Epochs." *Phil. Mag.* August, 1854, and, for further treatment, "Climate and Time."

‡ See KNOWLEDGE, No. 11, p. 218, for an admirably clear explanation of this complex movement by the Editor. Cf. article by Mr. Burr, "*Intellect. Obs.*," vol. iii, pp. 354, *et seq.*

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England and Belgium had been explored, but it was not until 1847 that the Torquay Natural History Society embodied the results of their labours in Kent's Hole, in a paper which was sent to the Geological Society. What obtuseness to the momentous revolution in current beliefs as to the antiquity and primitive state of man which these and like discoveries involved, the Council of that learned body displayed, is shown in this laconic entry in their Quarterly Journal, "On Kent's Cavern, near Torquay. In this paper an account was given of some recent researches in that cavern by a committee of the Torquay Natural History Society, during which the bones of various extinct animals were found in several situations."

Nor did they manage these things better in France. In the same year (1847) M. Boucher de Perthes called attention to the discovery of some rudely-shaped flint implements in pits which were being worked for sand and gravel in the Somme valley, near Abbéville. They had been found at intervals during the preceding six years in such positions and so far below the surface as to convince him that they were not later, but probably much earlier, than the deposits in which they were embedded, and in which were also found bones of the mammoth, woolly-haired rhinoceros, and other extinct animals. M. de Perthes argued that these worked flints had been fashioned by man, and witnessed to his high antiquity and low level of culture. But he was met with the reply that these so-called tools and weapons were either natural fractures or forgeries, and an account of similar finds of "instruments en silex" in the Drift at St. Acheul (hence the term Acheulian), near Amiens, which was published by Dr. Rigollot in 1855, met with the same reception.

It seems strange to us, with whom the "Origin of Species" has for some years been a canonical work, that, until within the last quarter of a century, even the masters in our scientific Israel were so fettered by traditional opinions concerning man, that they deprecated any resistance to these, so that the investigation which he had for a long period extended to the earth beneath him, and for a still longer period to phenomena above him, was applied to his kind and its place in the succession of life last of all.

A dozen years passed before *savants* on both sides of the Channel confessed themselves mistaken. In 1858-9, some English geologists, stimulated by discoveries in Brixham cavern, examined M. de Perthe's collection of implements, and the beds in which they were said to have been found. "In addition to being perfectly satisfied with the evidence adduced as to the nature of the discoveries, they had the crowning satisfaction of seeing one of the naked flints still *in situ* in its undisturbed matrix of gravel, at a depth of seventeen feet from the original surface of the ground."* An impetus was thus given to further research, and not only were discoveries of similar implements (presumably, from their general resemblance of form, of the same age, and shaped by the same race of men), made in England in beds of gravel, sand, and clay, for the most part on the slopes of our existing river-valleys, but it was ascertained that flint implements had been disinterred at the end of the last century from the Waveney Valley, in Suffolk, only to be, as it were, re-interred in the Museum of the Society of Antiquaries. The earliest known find of a flint in the drift was in the Thames Valley, probably at the close of the seventeenth century. It is said to have been found with the tooth, or, according to another account, the skeleton, of an elephant, near Gray's-Inn-lane, and is preserved in the British Museum. It would be easy to convert this paper into a dry catalogue of discoveries, and to avoid that prosaic result, it suffices to say that implements of stone,—leaf-shaped flakes, removed from flints by blows or pressure, and apparently intended as knives and scrapers; pointed weapons analogous to lance or spear-heads; oval or almond-shaped weapons, with cutting edge all round,—have been found by thousands in the drift of England and the Continent. This river-drift is formed of alluvial deposits bought down by that unresting, yet unhasting action of rain and flood which is for ever deepening the bed over which the waters flow. Since the time when the men of the Acheulian period lived in France, the Somme has cut down its valley one hundred feet—a result which requires an enormous antiquity for the flint implements found in the undisturbed gravels. The bottom of that valley has yielded polished stone weapons and other remains further illustrating the vast lapse of time between the Ancient and Newer Stone Ages—vast, even after making full allowance for a more rapid action of rain and flood in the Quaternary period than now.

* Evans's "Ancient Stone Implements," p. 478.

THE AMATEUR ELECTRICIAN.

ELECTRIC GENERATORS (*Continued*).

IN the previous article we described the first principles of magneto-electricity, and got so far as to say that when an electro-magnetic coil is made to pass across the poles of a permanent magnet, currents of electricity are induced in the coil. These currents are reversed in direction every time the coil changes its position in relation to the magnet, that is to say, the current induced as the coil approaches the magnet being in one direction, the current induced after the coil has passed, and as it recedes from the magnet, will be in the opposite direction. Commutators or current reversers are used, by means of which these opposite currents are sent in one common direction through the external circuit.

The electro-magnetic coil, however, need not necessarily be of the orthodox form. M. Gramme, about 1870, designed a coil (which, by-the-way, is called the armature of the large or field magnets, consisting of a ring of soft iron, with insulated copper wire wound round it in sections. The object in view was to have a part of the coil always passing through the magnetic field, and so to be constantly producing electric currents, instead of only once or twice in each revolution. For an amateur, however, the Gramme machine is comparatively difficult to make.

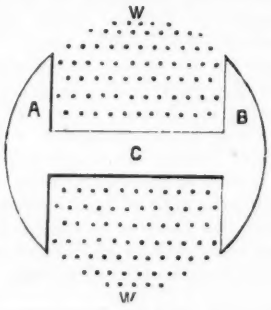

Fig. 1.

We prefer, therefore, to describe in detail a machine with what is know as "Siemen's" armature, being much easier to make, and quite as efficient. This armature

is represented by Figs. 1 and 2. Fig. 1. is a vertical section (full size), while Fig. 2 is a longitudinal section a little less than half-size. The dimensions here given are such as will produce a small instrument of great service for purposes where only a few cells are otherwise required. Larger machines can be described subsequently.

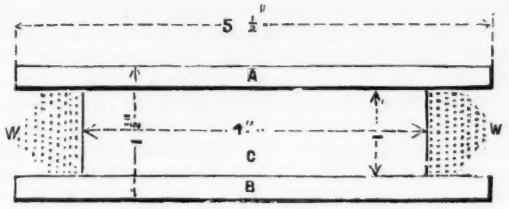

Fig. 2.

The armature is simply an electro magnet with the core flattened out, and its extremities extended so as to form pole-pieces of a segmental section. The core, C, in our machine is four inches long and one inch wide, the thickness being not more than three-eighths of an inch. The pole-pieces, A and B, are five and a-half inches long, quarter of an inch thick in the thickest part, and turned up so as to form parts of a circle of one and a-half inches diameter. The armature may be made in three pieces, and fastened together by means of wrought-iron screws or rivets; or, what is better, it may be cast in one piece, great care being taken to ensure that the iron is perfectly soft, and possesses no coercive force—that is to say, it must not retain any magnetic polarity after a current of electricity has passed round it. It must also be pointed out that when in working order, the armature has to revolve between what are called the magnetic pole-pieces bored out cylindrically, the greatest effect being obtained when the armature revolves in a circular boring only sufficiently large to allow it to move without touching. Consequently, the more true we make the curves of the pole-pieces, the more effective will the apparatus become. For the benefit of those of our readers who have not the facilities for turning out such work, we have deposited a pattern with a very good firm of iron-founders, who will send the casting, either in the rough or finished state.*

Well insulated copper wire should be very carefully wound round the core, so as to fill up the hollow portions of the casting, and form as nearly as possible a circle with AB, as illustrated by WW in Figs 1 and 2. It is recommended to use number 24 BWG, silk-covered (price 3s. 3d. per pound). Both extremities of the wire may be brought to one end of the armature. Before winding on the wire, gun-metal caps should be fitted ready to be fixed on to the armature. Fig 3 represents one of these caps in vertical section. Holes should be drilled in the face of each cap, and corresponding screw-threads tapped into the pole-pieces of the armature, so that perfect rigidity may, on screwing together, be ensured. The caps should also be furnished with projections (about half-an-inch thick and an inch in length), whose function is to act as a spindle. A small pulley wheel, say an inch in diameter, should be driven (or cast) on to one spindle projection, and a little ebonite tube over the other. In this latter cap two small holes should be made near the spindle, and through them the ends of the armature wire should be passed. What becomes of them we will say hereafter.

When working, the armature will revolve at the rate of 1,500 or so revolutions per minute, and it is therefore essential that everything should be true. The wire has naturally a tendency to fly out and will do so, unless two or three small grooves are made in the pole-pieces A, B, in

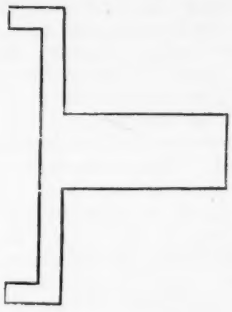

Fig. 3.

which a few turns of silk thread, or any equally tenacious non-magnetic substance, are wound, passing round the armature, and so securing the wire. It is as well, perhaps, to remind our readers that they should make sure that the entire armature is evenly balanced. This they can easily find out. We will leave our description of the other portions of the apparatus till next week.

* Readers wishing for these castings, can get them by sending to Mr. Benjamin Slater, Wellington Foundry, Charles-street, City-road, London, E.C., for KNOWLEDGE casting, No. 1, rough, 6d.; or finished, 3s.

PHOTOGRAPHY FOR AMATEURS.

BY A. BROTHERS, F.R.A.S.

PART V.

THE solution required for developing the negative is composed of protosulphate of iron 1 oz., glacial acetic acid 1 oz., alcohol 1 oz., and water (rain or distilled always to be understood in making up solutions) 20 oz. In hot weather more acid and less iron may be used. The iron should be dissolved before the acid and alcohol are added. Filtration is not necessary if the solution be allowed to settle.

When the plate is removed from the camera slide or carrier, it presents the same appearance as when placed in it (the film of iodide of silver in the collodion is creamy white); no image whatever is visible; it is *latent*, and requires to be "developed." Several agents are used for this purpose, pyrogallic acid and protosulphate of iron being most commonly employed. Development with pyrogallic acid is very slow as compared with the iron, and for that amongst other reasons the iron is mostly used. The light has caused a chemical change, and has so modified the silver compound that when the reducing agent is poured over the plate the silver is thrown down in a metallic state on those parts which have been acted upon by the light, in exact proportion as it has been reflected by the object which has been copied. This will be seen to be the case when the negative is examined by transmitted light; the lightest parts will appear most opaque, and the deepest shades show only bare glass. That the image is formed of metallic silver may be proved by gentle friction on the dry surface of the negative, as, after removal of the powdery surface, the polished silver will be found beneath.

In developing the picture, only sufficient solution should be used to cover the plate, and this, of course,

XUM

is determined by the size of the glass used. The developing solution must be caused to flow evenly over the plate by pouring it on at the lower edge, and at the same time by so accommodating the position of the plate that the solution will flow in one sweep to the top, and care should be taken to allow as little as possible to run off. The reason for this is that the silver is required to form the picture, and if washed away the image may be weak. If the proper exposure has been made, the image will soon appear, and its development must be carefully watched, in order to avoid too much intensity. The solution must be kept in gentle motion while on the plate, and may be returned to the glass cup while the plate is examined, and if the development has not been sufficient, it must be again poured on. Experience alone can determine this, and it will quickly be gained. What is required is sufficient intensity to give the proper light and shade *in the print on paper*, which is the object in view in taking the negative. Usually, when iron is used as the developing agent, the image is not sufficiently *dense* for printing from, and, when this is the case, it must be *intensified*. The solution for this must contain 15 grains of pyrogallic acid, 5 grains of citric acid, and water 5 ounces; and it is used by taking sufficient of the solution to cover the plate and a few drops of silver solution (20 grains, for water 1 ounce). This is used in the same way as the developing solution, and the effect on the negative must be watched to avoid over-intensity. After use, the developing and intensifying solutions may be put into any convenient receptacle when the silver they contain may be recovered in a metallic state.

The negative image is still veiled by the unreduced iodide of silver, and this can be removed by pouring over the plate a weak solution of potassium cyanide which will clear the plate, leaving only the metallic image. As potassium cyanide is a dangerous poison, hyposulphite of soda is preferred by some operators; its action is not so energetic as cyanide, but it answers the purpose equally well. A saturated solution should be used—that is, sufficient of the hyposulphite of soda must be put into the water until it will dissolve no more, the surplus crystals may remain in the bottle, and the solution may be repeatedly used.

The negative must be carefully washed after each of the operations we have now described, and if hyposulphite of soda be used for fixing, the washing must be more thorough than when cyanide is used.

If allowed to dry spontaneously, the plate must be turned face to the wall, or it may be at once dried over a spirit lamp, or by a fire; care being taken to avoid dust. In the experimental stage, the negatives need not be varnished before they are printed from. Before varnishing, make the negative so hot that it is just bearable when touched on the back of the hand, then pour on varnish as if it were collodion, returning surplus to stock bottle. Dry by fire, and the negative is complete.

The Imperial Commission of German Scientists, who will come to America to observe the transit of Venus on Dec. 6 next, will establish posts of observation in Charleston, S.C., and in Hartford, Conn. The Legislature of the latter State has adopted a resolution extending its courtesies to the distinguished visitors, and granting permission to erect on the State Capitol grounds a temporary building for their scientific apparatus.

A naturalist says there is an "ant town" in the Alleghany Mountains. It consists of sixteen or seventeen hundred nests, which rise in cones to a height of from two to five feet. The ground below is riddled in every direction with subterranean passages of communication. The inhabitants are all on the most friendly terms, so that if any one nest is injured, it is repaired by help from the other nests. Foreign ants of the same species are not tolerated.

HANLAN AND TRICKETT.

By an Old Club Captain.

I WAS able to study the action of these two scullers under favourable conditions on Monday, hoping to find evidence bearing on the question of rowing styles. But the superiority of Hanlan was manifestly not due entirely, or even chiefly, to any differences such as I touched on in my discussion of the styles in vogue at the two Universities. Yet the characteristic feature of Hanlan's rowing illustrates the principles I then considered. This feature is his marvellously scientific method of sliding; for he so uses the slide as to combine the effect of slide, swing, and arm work, thus bringing arms, back, loins, and legs into simultaneous action, so that his boat seems as if driven from a catapult. Then, so perfectly is each stroke given that there is not the slightest perceptible dip, and therefore no force lost in vertical pressures, and as little loss of way as possible between the strokes. One feature in Hanlan's build surprised me. I had been told that his muscular development is singularly well-proportioned, that there is no abnormal or unusual show of any muscles, but that all the principal muscles of the body are well developed. Also when I met him at Toronto, I had failed to recognise the peculiarity to which I now advert—viz., a most abnormal development of the deltoid, as compared with the biceps muscles. I venture to say that Hanlan is an anatomical marvel in this respect.

THE NAVAL AND SUBMARINE EXHIBITION.

THIS exhibition, which formed one of a course of what may be called "trade exhibitions," at the Agricultural Hall, Islington, and which has been held during this month, was, in the opinion of those old enough and able to judge, by far the best of its kind which has taken place since the Great Exhibition of 1862. Certain it is that the processes and appliances exhibited by some 500 or 600 firms are fraught with extremely great interest, not only to those in the various trades represented, but also to the general public. It is doubtful if anything can be conceived of greater importance than the saving of life at sea. Numerous systems having this object in view were offered to the public, embracing life-buoys, belts, boats, &c., and in addition to the ordinary boats, there were to be seen a number of devices, more or less ingenious, for compactly showing such vessels, yet in such a manner that they could be lowered in an almost inappreciably small space of time—time, in fact, which could be counted by seconds. In one case, the boat, made of canvas-like material, &c., could be doubled up longitudinally, and placed by the side of a ship, occupying only a fifth of the space it would fill when laid open. There were also several arrangements for releasing the boats from the davits, the best, perhaps, being that which allowed the hooks to drop away as soon as the boat touched the water.

Diving apparatus occupied a very prominent place; the most important exhibit in this department being that of Messrs. Fleuss, Duff, & Co. This apparatus is as useful for searching expeditions in mines or other places filled with noxious gases as it is for ordinary diving purposes. No pipes are used in this system, but the diver, carrying with him a small vessel holding a supply of oxygen gas sufficient to last for four hours, breathes his own breath over and over again, the exhaled air being passed through a filter containing caustic soda, which robs it of its poisonous exhalations.

Asbestos is a substance which receives a daily-increasing attention, and was exhibited in a great number of applications. It is expected—and it is to be hoped the expectation will not prove an unfounded one—that asbestos will very shortly be practically demonstrated to be one of the, if not *the*, best of electric insulators. Specimens were shown in which it took the place of ebonite, at something like one-tenth the cost. As was known very many centuries ago, it is a mineral which is fire-proof, and it is not to be wondered at that great efforts are being made to demonstrate its non-inflammability when used as a paint.

Another series of exhibits of very great interest were the materials devised for speedily, very efficiently, and economically removing paint. Altogether, the Exhibition may certainly be described as one of the greatest successes of the day.

CHARLES DARWIN.

THE death of Charles Darwin, which, even at the age of seventy-three, appears premature, will send a thrill of grief through the whole civilised world. No man during the present century has been better known, more quoted and misquoted, appreciated and scoffed at, than the author of that "epoch-making" work, "The Origin of Species;" and yet the man who thus set the world ablaze has been content to lead the life of a true philosopher—allowing friends and foes to say their say, and leaving it to his works themselves to justify praise and to refute calumny. Unambitious and unassuming, he has never thrust himself before the public, nor sought for honours and emoluments. He worked for the love of science and of truth, careless of his own reputation if only he could impart to others that which his own mind had grasped so firmly and analysed so accurately. As a naturalist, not even his greatest enemies will deny him the meed of praise. No other man could have drawn so much knowledge from a single scientific voyage, and the works consequent upon his connection with the expedition of the *Beagle* would have stood out as monuments of vast genius and unparalleled industry, even had he never written those better-known and much-criticised books which have made his name the war-cry of opposing factions.

The great thinkers of the day have long ago made up their minds as to the truth of Darwinism, although Darwin himself would have been the first to admit that the theory he advanced was still imperfect. Having given the bold, broad outline, he has left it to other workers to fill in the details; and even if, in so doing, it should be found necessary to efface a line here and there, he would have been the last to object to such effacement if it should prove desirable in the cause of truth. And meanwhile, the great and childlike philosopher, who thought not the lowly worm and clinging plant beneath his notice, has passed away quietly and unostentatiously as he has lived, few even knowing of his death till a full day after it had taken place. Nevertheless, his loss will be deeply and widely mourned, and the gap left by it in the ranks of science will long remain vacant, for in the present generation who could be found to fill the place so long occupied by Charles Darwin? As a stranger, I can testify to his great courtesy in replying to queries which must often have seemed frivolous; but the truly great man is always tolerant, and willing to give freely to others knowledge acquired with much pains and labour, and in this respect Darwin was truly great, and never despised or rejected anything placed before him which had the slightest scientific value.

It is not necessary here to give a list of his works; they must be well-known, at least by name, to all readers of KNOWLEDGE, and those who know them by name only would do well to make themselves familiar with their contents before they condemn the greatest philosopher and most profound thinker of the age, who, in his quiet country home, has worked out problems in zoology, botany, and geology, which otherwise would have remained, as the riddle of the Sphynx, fatal to all who attempted their solution.

A. W. BUCKLAND.

M. PASTEUR.

FOR thirty years M. Pasteur has carried on the most minute and elaborate researches into the lowest forms of life, and his discoveries, in the opinion of many, have established beyond all reasonable doubt the great fact that there is no such thing as spontaneous generation. He is the foremost representative of the "germ theory" of disease, and has absolutely proved in certain departments, and left it a matter of sure inference in others, that animal maladies may positively be traced to the presence of minute organisms in the body. There has been fierce controversy on these matters. There still are some vigorous opponents who refuse to be converted, such as Dr. Charlton Bastian, who held debate with M. Pasteur at the Congress last year; but there is no question as to which way the balance of opinion now lies, if, indeed, it is not incorrect to speak of the germ theory as being any longer within the sphere of opinion. The great advance that it has made towards certainty during the last few years is primarily due to the work of M. Pasteur. He did not, of course, invent the theory. It is in its outlines as old as the beginnings of scientific medicine; and in a somewhat advanced form it is as old as the last century. But M. Pasteur has given it at once a width and a universality that it lacked before, by his researches into the nature of fermentation and his microscopic studies of disease. It might be thought that beer was too everyday a subject for the investigations of one of the profoundest observers of our time; but M. Pasteur's work on beer has not only made the fortune of the brewers who were wise enough to read him, but has revealed the most important truths as to the mysterious process of fermentation. Wine and silkworms have also attracted his attention; so have chickens and sheep. It was, indeed, with the diseases of these two last that he was concerned in the memorable address last August. Chicken-cholera and splenic fever are mysterious and, it had been thought, incurable diseases. To M. Pasteur they have proved neither mysterious nor incurable, for he has found out the two facts, so important in themselves, so immeasurably important in their bearings on all similar diseases, that these forms of sickness are both caused by the presence of minute alien organisms in the body of the animal, and that they can be cured or prevented by a process analogous to vaccination. Vaccination, indeed, which has heretofore been regarded as a certain but inexplicable safeguard in one disease alone, is now in a fair way of being scientifically explained, and, as a consequence, of being proved useful in innumerable cases hitherto thought to be beyond its reach. Some of our readers will remember the statistics which M. Pasteur gave last year of the effects of the vaccination of sheep according to his method. May we not suppose that a similar cure is about to be discovered for the other plagues, whether of human or of lower forms of life, which are one by one being brought within the scope of the germ theory? The researches of Dr. Koch with regard to tubercular consumption, which Professor Tyndall explained in our columns a few days ago, are a case in point. Who can say whether in a few years, or in the next generation, at all events, it may not be the practice to vaccinate for consumption, as we now vaccinate for small-pox?—*Times*.

DEVELOPMENT IN FOOT-RACING.

THE winner of the six-day "go-as-you-please" contest, which began in New York, Feb. 27, made the unparalleled record of 600 miles. The second in the race covered 577 miles, beating every previous score save his own of 582 miles made in this city a year ago. The winner, Hazael, was on the track a few minutes short of 106 hours.

The scores made by the winners of the various six-day contests that have taken place since 1878 stand as follows:—

		Miles.
O'Leary...	Astley Belt, London, March, 1878	520½
Rowell ...	Astley Belt, New York, March, 1879	500
Weston ...	Astley Belt, London, June, 1879	550
Corkey ...	First race, Championship of England, 1878...	521½
Brown ...	Second race, Championship of England, 1879	542
Brown ...	Third race, Championship of England, 1880..	553
Hart	Rose Belt, New York, September, 1879	540
Murphy ...	O'Leary Belt, New York, October, 1879.....	505⅛
Hart	O'Leary Belt, New York, April, 1880	565
Rowell ...	Astley Belt, London, November, 1880.....	566
Panchot..	O'Leary Belt, New York, March, 1881	541½
Hughes ...	O'Leary Belt, New York, January, 1881.....	568½
Fitzgerald	Ennis Race, New York, December, 1881.....	582
Hazael ...	Contest at Madison Square Garden, March, 1882	600

In the last race, Rowell, who broke down, ran on the first day 150 miles in 22½ hours, the first 100 miles being covered in 12½ hours.—*Scientific American*.

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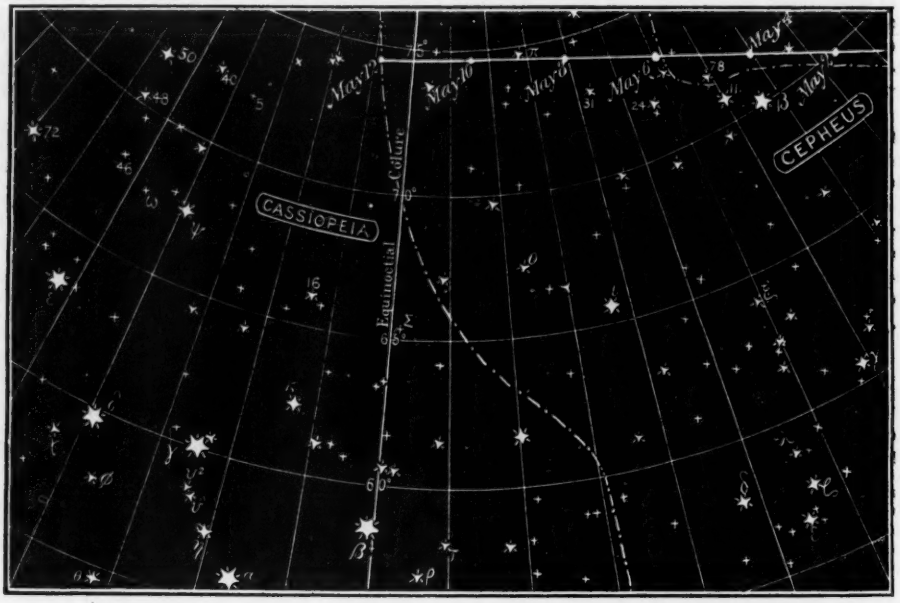

THE COMET.

WE give this week the path of comet Wells to the end of next week. We had prepared a map from the orbit given in the circular of the *Science Observer*, Boston; but this was incorrect, and we instructed the engravers not to proceed with the map. The path here shown is taken from the positions given by the superintendent of the *Nautical Almanac*.

THE NEBULA IN ORION.

PROF. HENRY DRAPER'S PHOTOGRAPHS OF THE SPECTRUM.

THE nebula in Orion is, for many reasons, a very interesting body. It is an immense mass of glowing gas, and presents the same condition that, according to the nebular hypothesis, the solar system did before any planets had been formed. Among these points of interest none is greater than that bearing on the chemical question of the non-elementary character of the so-called elements. If we examine the spectra of the sixty-eight elementary bodies found on the earth, and group them together, the resulting map contains thousands of lines; if we look at the spectrum of the sun, a considerable proportion of these lines are found there; if we observe a certain series of stars, the number of lines diminishes, till in such stars as Vega only about a dozen lines are seen; and finally, if we turn our spectroscope to the true nebulæ, only three or four lines remain. Such observations indicate that we are tracing back a process of evolution of the elements, and that from extreme simplicity complexity is being evolved. The evolution of the elements proceeds in the same way as the evolution of organised structures, the heterogeneous from the homogeneous.

The investigation of the compound nature of the elements has for some years attracted the attention of advanced scientific men, and various attempts have been made to solve the question. Among these we may now mention those of Professor Henry Draper, who has looked at the problem from the astronomical point of view. As the main hope for the dissociation of our so-called elements is from the influence of heat, it is obvious that in the heavenly bodies, where the temperature is far higher than any we can attain here, we may detect such decomposition. For this reason, for many years, Dr. Draper has been prosecuting this research, and step after step of advance has been attained.

Inasmuch as hand-work is inadequate for the correct and ready mapping of spectra, it was plain that if photography could be applied, great advantages would arise. Accordingly, Dr. Draper has constructed a series of instruments of greater and greater delicacy for the successive steps of this research, and beginning with spectra of the sun many years ago, has photographed the spectrum of star after star, even down to the tenth magnitude, and, finally, during the past month of March he succeeded four times in photographing the spectrum of a nebula—the nebula in Orion. Such photographs require careful study before all that they contain can be explained, but they constitute an important advance. One result, however, is obvious, hydrogen declines to be decomposed, and maintains unimpaired its position as an element. Curiously enough, in the same month, the celebrated English physicist, Huggins, has also obtained a photograph of this same nebular spectrum.

Dr. Draper has also taken photographs of the nebula itself, so as to watch for changes in it, and observe whether the process of aggregation into stars can be detected. Collated with the photographs of the spectrum, they show clearly evidences of such condensations.—*New York Times.*

[Dr. Draper writes to me as follows:—"I do not get the line at λ 3,730, of which Dr. Huggins speaks in the April number of the *American Journal of Science*, but, on the other hand, I photograph *h* (λ 4,101), and a couple of other lines in its vicinity which he does not get. Moreover, I have found two curious condensations (in the nebula) which give a continuous spectrum, and indicate either compressed gas or liquid or solid. These just precede the trapezium, and do not show themselves as stars on the photographs of the nebula (without spectroscope)."—R. A. P.]

LITTRÉ.—Littré's faith in Positivism arose from the mental quietude it offered on the great metaphysical questions. Negation as well as doubt incommoded him. Auguste Comte extricated him from both by a dogmatism which abolished all metaphysic. Following this doctrine, M. Littré said to himself, "Do not trouble yourself either with the origin or end of things, with God, the soul, theology, or metaphysic." What quietude for this ardent mind, ambitious of traversing every field of knowledge. This quietude, however, has been misconstrued, and appearances have been deceptive in representing M. Littré as a staunch and resolute atheist. The religious creeds of others were not indifferent to him. "I know too much," said he, "of the sufferings and difficulties of human life to wish to deprive anybody of the convictions which bear him up in every trial." He no more denies the existence of a God than the immortality of the soul. He excludes the consideration of the notion from the mind, because he is proclaiming the impossibility of scientifically attesting it. For my part, regarding the words progress and invention as synonymous, I ask what is the new philosophical or scientific discovery which can remove such great anxieties from the mind? They seem to me essentially everlasting, for the mystery enveloping the universe, of which they are an emanation, is itself essentially everlasting.—"Pasteur's Address before the Academy of Science."

WEATHER REPORT, FOR WEEK ENDING SATURDAY, APRIL 29.

STATION.	ABERDEEN.							LIVERPOOL.							VALENCIA.							LONDON.							LYONS.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max.	52	55	54	53	47	49	48	58	54	53	49	52	53	55	56	56	53	52	54	54	53	59	59	59	54	53	53	57	—	62	60	—	52	—
Thermometer in Shade. Min.	40	40	40	39	36	38	39	47	46	42	44	41	44	41	45	46	41	40	40	43	37	44	47	40	38	37	41	39	—	42	46	—	41	—
Thermometer in Shade. Day's Range	12	15	14	14	11	11	9	11	8	11	5	11	12	14	11	10	12	12	14	11	16	15	16	19	16	16	12	18	—	20	15	14	—	11	—
Barometer. Inches	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	29	—	29	30	29	—	29	—
Barometer. 100ths	·30	·13	·19	·37	·67	·25	·23	·14	·43	·42	·38	·74	·06	·37	·48	·76	·41	·63	·71	·41	·27	·26	·50	·53	·34	·76	·23	·53	—	·96	·00	·63	—	·82	—
Wind. Direction	S W	W	N W	N	N W	S S E	N N W	E	W	W	N N W	S	S S W	W S W	N N W	W N W	E N E	E N E	W S W	N N W	N N E	S	S W	S S W	S N W	W N W	S S W	S S W	W	S S E	S S W	S S W	—	W	—
Wind. Force (0 to 12)	1	1	6	8	2	6	5	3	5	3	3	1	2	1	6	6	2	1	5	7	3	7	3	2	3	2	2	3	—	1	1	1	—	1	—
Weather. Beaufort Scale*	b c	c	b c	c	b	o p	r	m	c	o	b	b	b c	o	c q m	m	r m	o	b c o	r q p	r q q	o q p	b q m	b c c	c	b m	b c	o	—	b	o	b	—	o	—
Rain. 100ths of inch.	22	7	11	1	11	2	37	39	3	1	0	0	9	5	74	3	30	3	6	58	35	34	24	10	91	0	15	10	0	20	0	4	—	0	—

* WEATHER.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

NARCOTIC INDULGENCES.

OPIUM-EATING AND SMOKING, AND MORPHIA-CRAVING.

BY M.D.

OF late years medical men have become familiar with a new form of opium-eating—namely, "Morphia-Craving," designated by the Germans "Morphium-Sucht." Morphia is one of the "Alkaloids," or essential principles of opium; it possesses powerfully narcotic properties, it allays pain, and soothes the nervous system. Owing to its concentrated virtue, it has within the last few years come largely into use by the mode in injection beneath the skin. A small syringe, graduated for the measurements of the dose, is fitted with a fine tube of the size and sharpness of a needle. The injection of a dose, suited to the emergency of the case, is followed by almost instant relief of the most intense pain, and even if sleep be not induced, the most agreeable sensations of relief supervene. So seducing is the sense of ease and rest, under the influence of this mode of administration that, once experienced, the desire for its repetition recurs frequently, until it becomes a hard matter to resist or to overcome the craving for the drug. The solution, inserted into the loose tissues below the skin, is rapidly—almost instantaneously—absorbed into the current of the blood, and thus carried to the seat of pain.

We know of instances where patients have purchased for their own use the injection syringe, and have practised the use thereof for years—the breaking off of the habit having been found impossible. It is therefore not without some anxiety, and a deep sense of more than ordinary responsibility, that the medical man avails himself of so invaluable a therapeutic means. The effects of the prolonged use of the subcutaneous injection of morphia is to blunt all the nervous energies; to demoralise, in fact, the powers of the mind, and to make the supplementary indulgence in alcoholic stimulants an equally irresistible craving.

Opium-eating is prevalent in several districts in England. In Norfolk and Lincolnshire it is not casual or rare, but popular, habitual, and common, and supposed to be "protection against ague." A writer in the *Medical Times and Gazette* (July 19, 1873) says:—"Any one who visits such a town as Louth or Wisbeach, and strolls about the streets on a Saturday evening, watching the country people as they do their marketing, may soon satisfy himself that the crowds in the chemists' shops come for opium. They have a peculiar way of getting it. They go in, lay down their money, and receive the opium pills in exchange without saying a word. For instance, I was at Wisbeach one evening in August, 1871, went into a chemist's shop, laid a penny on the counter. The chemist said, 'The best?' I nodded. He gave me a pill-box and took up the penny, and so the purchase was completed without my having uttered a syllable."

Opium, there is little doubt, is largely and habitually consumed by persons in England. In the northern counties, more especially in Lancashire, there is a nostrum extensively sold under the name of Godfrey's Cordial, which contains a large quantity of opium. It consists of a decoction of sassafras-wood, with some treacle, and the addition of opium in uncertain quantities. This villanous compound is sold in large quantities, and used by mothers engaged in factory work to keep their infants asleep and quiet whilst they are from home. The mortality among infants in these districts is frightfully large.

It would be a needless occupation of your space to repeat the stories of opium smokers and eaters in China and other parts of the world, but one point should be noticed. The quantity used at each smoking "bout" is very small—exceedingly so to begin with. Taken in the solid form in small doses, as done by some of the runners and carriers of India, it is found to be more sustaining and far less injurious than alcoholic stimulants. That it is a deadly poison cannot, however, be controverted by such facts. Two wrongs don't make one right.

It certainly was a strange oversight that in the last Act of Parliament restricting and regulating the sale of poisons, opium, in a crude or solid state, was omitted from the schedule, while paregoric, a very mild opiate, was included, and cannot be sold without statutory precautions; while opium pills, or powdered opium, may be purchased without restrictions. The writer knows an instance in which a young man bought in a druggist's shop, without any questions asked, enough opium to kill himself. At the inquest, the druggist escaped censure, because the drug was not entered on the schedule of the Act for the Regulation of the Sale of Poisons.

In illustration of the danger of the reckless use of opium, I would add the following incident:—

I was one evening hastily summoned by a person who told me that my son was in a fit in an adjoining street. I hurried off in the direction indicated, in a state of the most painful anxiety, as he had never previously had a fit. On my way, I met two policemen with

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my son on a stretcher, and I accompanied them to the nearest station. My son was totally unconscious, and his countenance had the paleness of death. His limbs were flaccid; his breathing slow, regular, and performed without effort; his pulse was soft and slow.

The report to me by the constables was, that they had found him in this state, seated on the pavement, propped up against a wall. The impromptu and inevitable verdict of most of the police was "Drunk." There was, however, no smell of alcoholic liquor in his breath; neither was his physical state like that of a man "in drink." One of the constables, more intelligent and cautious than his fellows, pointed out these features, and said "He is not drunk, but he's drugged." I hesitated to accept this explanation, since he, being a medical student, I thought, would not easily be "hocussed" in that manner. Nevertheless, the sequel proved the man was right in his diagnosis.

As I was well-known in my professional character at the station, I was allowed to remove my son home. He was placed in bed and kept quiet. In the course of about eight or nine hours he woke up, and was at first quite at a loss as to his "whereabouts." After an attack of vomiting, he recovered his faculties, and gave the following account of his illness.

He and a fellow-student, discussing the subject of opium-smoking, in a thoughtless moment agreed to experimentalise upon themselves. They were both tobacco-smokers. They placed each a small pinch of powdered opium in their pipes with the tobacco and smoked away for a short time, when they began to feel agreeably intoxicated. They went to an adjacent tavern and asked for coffee; whilst this was being prepared they fell asleep on sofas. After some time they were ejected from the inn as being the worse for liquor. They wandered out in the street; my son remembered getting into an omnibus, but forgot everything afterwards until he came to himself at three o'clock in the morning. Upon close inquiry, I learnt that the conductor of the omnibus when he got to his journey's end turned his passenger out, and saw no more of him. His fellow-smoker could give no account whatever of his doings until he found himself in bed, in his lodgings, the next morning. In these two young men the effect of the inspiration of opium fumes was a dreamless sleep, with some of the phenomena of somnambulism. A slightly stronger dose would, doubtless, have sent them into that sleep from which there is no waking.

The narrative here given may, perhaps, warn others from such experimenting, and it should also serve as a caution against opium smoking to any extent at all.

SEAL "FISHERIES."

A LARGE part of the world's supply of seal-skins for furs comes from the Prybilov Islands, off the coast of Alaska. On two of these islands the seals congregate in vast numbers early in spring, and remain for three months. With the exception, I understand, of one island off the coast of Japan, the fur seal is nowhere else seen in the North Pacific, and it is said that the seals on the coast of Alaska are colonies which migrated suddenly from the other island. Be this as it may, the habits of the seal prove that it has a remarkable attachment to localities. The sealing is farmed by the United States Government for, I think, £50,000 a year to the Alaska Commercial Company; and an agent of the United States resides on the principal island and superintends the annual slaughter of a stipulated and selected number of victims. By this precaution there is little danger, from man's inconsiderate rapacity, of the fur seal being exterminated in the North Pacific. Some years ago I heard Mr. Bryan, the Government agent, describe to the California Academy of Sciences, the fur seal trade carried on under his supervision. With only a variation of a few days in point of time, the males arrive in a great shoal that agitates the sea as far as the eye can scan, and, hobbling across the beach, establish themselves at "home" on the ledges of rock, the strongest males always taking possession of the best places. Next day, or, at the outside, on the second day after the arrival of the males, the females are discerned on the horizon, and ere they have emerged from the waves, the males have rushed down to the beach to welcome them, which they do with much roaring and quarrelling among themselves. Fighting for possession of the females is legitimate warfare, and in such a contest the greatest number of females fall to the most powerful males, some of which secure five or six companions. A day after the female lands, she gives birth to her young (one or two), and hardly have the offspring begun to suckle when she receives the embraces of the male. During their stay on the islands, combats of great ferocity occur among the males, and the older animals exhibit scars and gashes which render their pelts of little value to the furrier. A short time before the seals go to sea again, the males and females separate, and are to be seen extended in long lines apart from each other along the beach. The young stay with the females, and go to sea with them. It is a most curious fact that, during the whole of the three months on the islands, neither male nor female enters the water. They abstain entirely from food throughout the whole of the period, and it is almost unnecessary to say that, although they arrive in plump condition, they return to their fishing very lean. The animals are killed, and not skinned alive. Males alone are killed, and these only at three years of age. After that age, their combats materially reduce the value of their skins. As soon as the males have separated from the females, the fur-providers' operations begin. Day after day, groups of the males are surrounded and cut off from their companions, and a grand drive of the game takes place towards the curing establishments. On the way thither, the males which are unsuitable are allowed to slip seaward between the ranks of the drivers. On reaching the slaughtering-ground, a blow or two from a club on the head kills the animal. The skins are removed with the greatest care, in one particular way, and are given to the dressers, who scrape and rub down the inner parts, leaving them soft, and remove also the outer rough hair, which completely covers the inner fur. Some precautions of curing having been taken, the skins are periodically shipped by the company's steamer to San Francisco. There they remain some time in the company's warehouse in Sansome-street, but are all ultimately sent to London to be dyed; and, as I have been informed by the officials of the company, the dyeing operations are a secret in the possession of one family in London. It thus happens that seal-skins in the United States, on re-admission to the country, have to pay duty as a manufactured article, and are much more costly than in England. Occasionally, dyers in San Francisco experiment to get at the secret of the dyeing in London, but no one has succeeded. Some dyeing, I believe, is performed in Germany, but it is possibly by a branch of the London establishment. D. J. McR.

A DOG GOES OVER NIAGARA FALLS ALIVE.

A LARGE dog lately survived the passage over Niagara Falls and through the rapids to the whirlpool. He was first noticed while he was within the influence of the upper rapids. As he was whirled rapidly down over the falls, no one imagined but that that was the last of him. Shortly afterwards, however, he was discovered in the gorge below the falls vainly endeavouring to clamber up upon some of the *débris* from the remains of the great ice bridge which recently covered the water at this point, but which had nearly all gone down the river. The news spread rapidly through the village, and a large crowd gathered on the shore. Strenuous efforts were made to get the struggling animal on shore, for an animal which had gone safely over the falls would be a prize worth having, but without success. Finally the dog succeeded in getting upon a large cake of ice, and floated off upon it down towards Suspension Bridge and the terrible whirlpool rapids. Information of the dog's coming was telephoned to Suspension Bridge village, and a large crowd collected on the bridge to watch for the coming wonder. In due time the poor fellow appeared upon his ice-cake, howling dismally the while, as if he appreciated the terrors of his situation. An express train crossing the bridge at the time stopped in order to let the passengers witness the unusual spectacle. Round and round whirled the cake, in a dizzy way, and louder and more prolonged grew the howls of the poor dog. As the influence of the whirlpool rapids began to be felt, the cake increased in speed, whirled suddenly into the air, broke into two, and the dog disappeared from view. No one thought that he could possibly survive the wild rush through the rapids. When, therefore, word was received that the dog was in the whirlpool, still living, and once more struggling vainly to swim to land, it was received with marked incredulity. This story was substantiated by several trustworthy witnesses. It seems incredible that an animal could go through the upper rapids, over the falls, through the gorge, through the whirlpool rapids, and into the whirlpool itself, a distance of several miles, and still be alive. The poor animal perished in the whirlpool.—*Scientific American.*

"In two instances dogs have been sent over the Falls and survived the plunge. In November, 1836, a troublesome female bull terrier was put in a coffee sack by a couple of men who had determined to get rid of her, and thrown off from the middle of Great Island Bridge. In the following spring she was found alive and well about sixty rods below the Ferry, having lived through the winter on a deceased cow that was thrown over the bank the previous fall. In 1858, another dog, a male of the same breed, was thrown into the rapids, also near the middle of the bridge. In less than an hour he came up the Ferry stairs, very wet and not at all gay. He was ever after a sadder if not a better dog."

"Niagara."—By George W. Holley.

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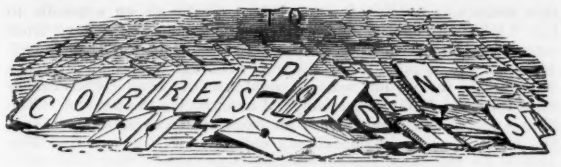

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

POPULATION OF THE EARTH: A CURIOUS PROBLEM.

[391]—A correspondent, James Connal, asks how much space would be required on the earth, on the assumption that, beginning with a single pair, the human race had multiplied during 5,000 years at the rate of 30 children to each pair, sons and daughters being born alternately, and each husband and wife being at the time of marriage respectively 21 and 20 years old—there being also no deaths. The problem is too difficult "for any use," as Americans say; but it may be roughly dealt with thus:—Children being born from the twenty-first to the fifty-first year of husband's life, and from the twentieth to the fiftieth of the wife's, we shall obtain a fair mean estimate, rather under than over, if we take 30 children for each pair at the age of 40 (nineteen or twenty being really born before and the rest after 40). On this assumption the population of the world in 5,000 years (or 125 times 40) would be

$$2+30+450+\&c.\text{ to }126\text{ terms}$$

$$=2\left[\frac{15^{126}-1}{15-1}\right\} =\frac{1}{7}(15^{126})\text{ approximately.}$$

Now log. 15 = 1·1760913, and multiplying by 126

log. (15^{126}) = 148·1875038
log. 7 = 0·8450980

log. (number representing population) = 147·3424058

= log. 2199915, followed by 141 digits, which digits we may represent without appreciable inexactness by cyphers. Now, assuming that ten persons could stand on a surface one yard square, 30 millions could stand on a square mile, and on the entire earth, whose surface is about 200,000,000 square miles, some 6,000 millions of millions. Dividing the number just obtained by this, we get 3666526, followed by 125 cyphers. It would require this inconceivable number of worlds like our own for the population on the assumptions made.

Let us see how large a single globe would suffice to give standing room to this population, ten to the square yard.

Such a globe must have a diameter exceeding the earth's as the cube root of the above quantity exceeds unity, or, roughly, as 7 followed by 43 cyphers exceeds unity. (In reality it will be larger considerably, but this is near enough in such a problem as the present.) Now the diameter of the orbit of Neptune, roughly, exceeds the earth's diameter nearly 700,000 times. Hence the diameter of the required globe exceeds the diameter of Neptune's orbit more than a hundred billions of billions of billions of times.

Supposing the farthest star visible in the great Rosse telescope to lie some thirteen or fourteen millions of times farther from us than the nearest, which lies about 70,000 times farther than Neptune, the distance of that star, 1,000,000,000,000 times farther than Neptune (or a light-journey of some 40 millions of years), would be but the

100,000,000,000,000,000,000,000,000th

part of the radius of such a globe as would be required to hold the population we are considering. A sphere having a radius equal to 100 millions of years' light-journey would not suffice even to *contain* so many human beings.

COD SOUNDS AND SCIENTIFIC PRIVILEGE.

[392]—Though I am glad to see Mr. Mattieu Williams' exposition of his views upon the subject of Cod Sounds, as it enables one to understand the way in which his (as I take it) misconception of the nature of that structure has been brought about, yet I am very sorry that any clumsiness of expression upon my part should have led him to think that I am an upholder of that paltry spirit of scientific exclusiveness which he with such vigour and justice condemns. Versatility in my eyes ranks next only to accuracy in a scientific man, and I agree entirely with Professor de Morgan that the ideal scientist should "know something of everything, and everything of something," indeed my own studies in biology have been undertaken mainly with a view to enlarging my grasp of another science from which I have borrowed the *nom de plume* under which I write.

This notwithstanding, I trust Mr. Williams will pardon my pointing out that, for instance, Pasteur's splendid acheivements "in the domain of the biologist" were not published as the recollections of work done in his student days twenty or thirty years previously. Since the days when Mr. Williams was a student of natural history, &c., at Edinburgh, biology has made vast strides, partly by the mere increase of the number of observers, but mainly by the aid of absolutely new methods of research with refinements in appliances undreamed of forty years ago. The grand generalisations of Darwin have, besides, directed inquiry along lines entirely new. Did Mr. Williams acquaint himself with the writings of modern Comparative Anatomists, and then perform one careful dissection of a "Cod-fish," I feel sure he would come to a very different conclusion as to the nature of the "Sound." As, however, it would be unreasonable to ask him to recommence the study of so large a subject after such a lapse of time, I will just give an account of the appearances presented in my recent dissections of the fish under discussion.

I would point out that, as a preliminary, the blood-vessels were injected with a brightly coloured fluid which made them so distinct as to leave no room for doubt as to their nature.

Just under the throat the heart is situated, and from it proceeds forwards a large vessel, the *ventral* aorta; this subdivides into a number of branches which distribute blood to the gills. After traversing the minute capillary vessels of those organs, the blood is carried into a corresponding set of large vessels, which, after giving off one or two unimportant branches, unite to form a medium trunk, the *dorsal* aorta, which almost immediately sends branches to the stomach, intestine, and various glands. From the point whence these arteries are given off, the aorta bends up and runs in direct contact with the underside of the backbone down to the tail. Lying beside the aorta are two large veins.

Beneath these blood-vessels is a large bag (the sound) with fibrous saccular walls, the dorsal (upper) wall of which is very thin and delicate, and the ventral (lower) and lateral walls exceedingly thick and strong. This bag extends through almost the entire body-cavity, and is quite shut. It terminates in front in two blind prolongations which lie loosely among the nerves and blood-vessels just behind the head. Within it (besides air, which I have always found) is a body known as rete mirabile, a closely-packed arterial network, which receives its blood from a very small branch of the aorta. Between the sound and the backbone are the kidneys, elongated bodies of a deep-red colour, which in the dead fish, at any rate, contain a great deal of blood. They are divisible into three regions—a thick *head kidney, not in contact with the sound*, extending from the hinder part of the skull backward to the *mid-kidney*, a much thinner portion, occupying the middle region of the body-cavity; and a very thick portion, the coalesced right and left *hind kidneys*, passing back beyond the hinder end of the body-cavity. The kidneys are supplied with blood by a number of small branches of the aorta, and their ducts (the *ureters*) pass right through the sound, perforating both its upper and under walls, and carrying side by side with them *a small artery*.

When the coloured fluid was injected into one of the large arteries, it passed freely into the aorta (the dorsal one I style simply *the aorta*) and all its branches, even to the most delicate; yet *not one drop found its way into the cavity of the sound.*

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The foregoing facts I can personally testify to.

Professor Hæckel classifies the true bony fishes (*Teleostei*) as *Physostomi*, or those in which the air-bladder communicates throughout life with the alimentary canal by an open duct; and the *Physoclisti*, in which the duct, though open in the young immature fish, is closed in the adult. To the latter group (which is further characterised, according to Professor Huxley, by the presence of a rete mirabile in the air-bladder) the cod belongs.

To sum up the case, I contend:—

1. That there is *air* within the sound, which, unless produced by *post mortem* changes, would by all analogy certainly cause the death of an animal if in its aorta.

2. There is also within it a rete mirabile which *might* be mistaken for a blood clot, but I have never found any blood.

3. There are, between the sound and the backbone, the kidneys, which, in Mr. Williams' student-days, *may* have been regarded by anatomists as mere clots of blood, and it is obviously to them that he refers as the "great dorsal blood clots," agreeing as they do exactly in distribution with the said "clots." It is quite contrary to my experience to find blood forming clots in *arteries*, though they are almost invariably found in the *veins* of dead animals, and it is more than doubtful if such definitely distributed ones could form in a large sac like the cod-sound.

4. The head kidney lies in front of, and therefore must be outside, the sound.

5. There is no communication between the circulatory system and the sound, as is proven by injection.

6. The sound has walls made up entirely of fibrous tissue, and therefore is not comparable to human arteries, which have muscular walls, as have those of all other animals. It could not supply any force for the propulsion of the blood, being merely *elastic*, and not possessing *contractility*.

7. There being no *systemic* heart in these bony fishes, the *dorsal* aorta cannot proceed from the *bulbus arteriosus*, as suggested by Mr. Williams.

8. The "sound" of the cod originates as an "off-shoot from the upper part of the digestive canal," just as the air-bladders of other fishes do, and therefore is the precise homologue of those structures; and, whatever its true function may be, it certainly plays no part in the circulation of the blood. Its strength is no obstacle in the way of comparison with undoubted air-bladders, as the carp (a much smaller fish) has an air-bladder almost as thick, with an open duct; and in the extinct genus, *Cœlacanthus* (a Ganoid), we actually find a *bony* air-bladder. Old Fossil.

BREAK FOR TWO-WHEELED VEHICLE.

[393]—I have a small pony, about 9 hands high, that runs in a small two-wheel ladies' basket carriage. He is a very useful and sagacious pony, but very frightened of going down hill, for fear he should fall with the vehicle on him. I am advised by our country cousins about here to whip him, and make him run down-hill. I think this advice both cruel and unwise. I think my pony knows as well as I do that he might fall. In order to mend matters, can any of your numerous correspondents say if they have had any experience with a break applied to a two-wheeled vehicle? and should be obliged by a description.

I am quite aware that breaks to four-wheeled carriages are common, but with two wheels the difficulty is, that wherever the break is placed on the periphery of the wheel, the effect is to press down the shafts on the pony's back. By applying the break to the nave of the wheel, some objection to this is removed, but I am told that it strains the wheels. And, also, should the break be on both wheels—that is double? W. H. C.

A PASSAGE IN "IN MEMORIAM."

[394]—I consider it to be utterly impossible for the allusion to be to Longfellow. Goethe is no doubt the poet referred to. Longfellow does *not* sing "in divers tones," whereas Goethe does. It appears to me that what was in Tennyson's mind at the time of writing was the manner in which Faust, in the second part of the drama, finally works out his own salvation. The allusion is probably one in spirit, rather than in letter, though it is possible that in the "conversations with Eckermann" some passages may be found to throw light upon it. Tennyson himself has, I believe, stated that the reference is to Goethe.

Next as to dates of publication. "In Memoriam" appeared in May, 1850 (though written years before), and the introduction is dated 1849. Longfellow's poem is avowedly based upon a passage in St. Augustine's "De Ascensione" (*De vitiis nostris scalam nobis facimus, si vitia ipsa calcamus*), shows here and there slight and unusual traces of Tennysonian mannerisms, and forms part of the First Flight of "Birds of Passage." So far as I can remember, this series was published about 1858, probably as an appendix to the "Miles Standish" volume. This much may be gleaned from internal evidence. The poems appear to be chronologically arranged. Those on the death of the Duke of Wellington and on Florence Nightingale, for obvious reasons, could not have been written earlier than 1852 and 1855 respectively. I cannot give the date of "The Two Angels," but it may easily be fixed by the event it refers to—the birth of one of Longfellow's children and the death of Mrs. Lowell on the same day. "Victor Galbraith" *may* have been written as early as 1846, and "The Jewish Cemetery" probably sprang from the poet's visit to England. I think that "Daybreak" appeared about 1856 (or somewhat earlier) in one of the magazines edited by Charles Dickens, but I have no present means of ascertaining. The series closes with "The Fiftieth Birthday of Agassiz," which is dated May 20, 1857, just seven years after the publication of "In Memoriam." I believe, therefore, that the resemblance between the passages in question is purely accidental, but that if it be otherwise, Longfellow, not Tennyson, must be held to be the copyist. I may add that the so-called parallel passages from Petrarch and Milton need hardly be taken into consideration.

George E. Dartnell.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—*1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 4. Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

R. Ouseley. If your communication were not quite so long! It is so difficult to abridge satisfactorily. No; the gentleman to whom you refer is not my son, nor have I a son old enough for the rank of colonel, unless thirteen years be considered a suitable age in India.—C. J. Brown. The answers to correspondents will not be abolished, but reduced in quantity. I am glad to think that *some*, at any rate, among readers have not misunderstood what was meant—in almost every case—for good humour. Several have taken for downright severity what I have meant for good-natured fun. I do so thoroughly enjoy banter (I mean when I am bantered myself), that I cannot quite understand how others can be hurt by it. To give an instance,—one correspondent, who really has some very wild ideas about stone implements—which he regards as natural formations—describes my jocular objections to his views as "worthy only of a palæolithic savage." You are right in thinking R. A. Proctor and the Editor of Knowledge one and the same individual. I never supposed it would be thought otherwise, when I mentioned that the Editor of Knowledge (R.A.P.) wished R. A. P., student of science, to go to Egypt to observe the eclipse—which task, by the way, the latter has been obliged, greatly to his regret, to decline.—Curiosity. Since noticed. Letters had been too multitudinous to be got through.—G. H. P. Egyptian centres, area, period, and duration of totality given this week.—J. S. Thanks for pointing out that the doggrel rhymes about the borrowing days do not occur in the "Complaint of Scotland," a prose work. Does it not seem as though the observed coldness of those three days, which in the sixteenth and seventeenth centuries were the first three days of April, affords a sufficient reason for the conceit that March had borrowed these days from April? in other words, that though April days in name they are March days in quality.—Decimal. The rule for placing the decimal point in division of decimals, simple and recurring, is given, I think, in most books on arithmetic. It is not very concise, but is simple enough. I have forgotten it. But in practice one does not need any rule of the kind. I simply set off the decimal point of division as many digits to right or left as may suffice to leave only one digit on the left of it; and then set the decimal point of dividend as many digits in the same direction, putting in zeros if any are wanted. It then becomes obvious where the decimal point of quotient must fall. Thus consider the following cases:—

First, 127·412)15·315(; I write this, or conceive it written, thus, 1·27412)·15315(. Here manifestly the quotient will begin ·1.

Secondly, 167·531)15·315(; I write this 1·67531)·15315(, and manifestly the quotient here will begin ·09.

Thirdly, 167·531)·015315(; I write this 1·67531)·00015315(, and manifestly the quotient will begin ·00009.

Fourthly, 127·412)1531·5(; I write this 1·27412)153150(, and manifestly the quotient will begin 1, followed by five digits before the decimal point.

Fifthly, ·0167531)1531·5(; I write this 1·67531)153150(, and manifestly the quotient will begin with 9 followed by four digits before the decimal point. The plan is not affected when either divisor or dividend or both contain recurring decimals. This is not, I must admit, an answer to your question for an exact (and you suggest) concise rule. But it is far better to have a common-sense plan, the *rationale* of which is obvious, than a mere verbal rule, the terms of which may be forgotten when the rule is wanted.—A FELLOW OF THE GASTRONOMICAL SOCIETY. I believe that for persons with your symptoms, Hanwell, Bethlehem, and Earlswood, are equally open; but as "great wit to madness nearly is allied," so that entire absence of wit precludes the idea of madness, Earlswood seems more suitable than the others.—A SUBSCRIBER. Certainly no new force is obtained by using coals to produce electricity, only a different form of force.—J. B. There is no correspondence between the Scandinavian deities and those of the Greek mythology. If Thor as the Thunder God corresponds with Zeus, Odin as Thor's father would correspond with Saturn; but Odin is in many respects akin to Jupiter. Sæter is regarded as the Anglo-Saxon deity for Saturday; but probably only from verbal kinship.—JAS. SMITH.—There is no such rigid arch. Tresca's experiments show conclusively that at a depth of less than twenty miles, the pressures are so great that the hardest solids would behave as fluids. He found steel perfectly plastic, and, as it were, viscous, under a much smaller pressure.—G. W. L. Soon I hope to deal with the reappearance of Biela's comet in another form.—IGNORAMUS. A misprint for earth, as you say.—J. TINSLEY. I do not know the exact size of Lincoln's Inn Fields. It is about 750 or 760 ft. from north to south, but less from east to west; the Great Pyramid's base is 760 ft. square. The area of the base of the Great Pyramid is about 13¼ acres, the area of Lincoln's Inn Fields about 12 acres.—ALGERNON BRAY. The fraction represents the exact chance; but the question belongs to the more difficult departments of probabilities.—A. McD. Even in the temperate zone there might occur a glacial irruption. It is evident the mammoth was suited to bear cold. Adhémar's theory has no scientific standing.—VOLO SCIRE. The co-efficient of expansion for iron will not give the law of increase in length of iron wire. Your question how to make an artificial bead resembling black pearl, I cannot answer. Possibly Lieut.-Col. Ross may be able to.—JOHN REID. There is no proof that as rock substances cool they continually diminish in bulk. Some substances would unquestionably behave like ice, floating on the molten matter. The point has been considered by Sterry, Hunt, Mallet, Dana, and others (myself, for instance). But of most rock substances (as distinguished from metals) it seems to be true that the crystalline products resulting from their slow cooling are of greater specific gravity than the fused rocks.

ELECTRICAL.

W. H. and others. There are several good text-books on electricity, but for the untechnical student, Deschanel's "Electricity and Magnetism" is perhaps the best. The Leclanché is the best form of battery for physiological purposes.—H. W. B. 1. The arc in the 150,000 candle-power Brush lamp is about 1·25 in. 2. Quadruplex has not been applied to either of the Transatlantic cables.—P. 1. Had one-tenth, or even less, of the current generated in Edison's dynamo passed through the body of the gentleman who placed his hands on the terminals of the machine, the result would doubtless have been fatal. It should be remembered that the current generated by the machine is of comparatively low tension, being what is called a large-quantity current (see our eighth article on Electric Exhibition), and the external circuit is proportionately low, therefore, when the hands are placed on the terminals, a branch circuit of 2,000 or 3,000 ohms resistance is offered to the current. The joint resistance of 1,000 lamps, at 100 ohms each (omitting the resistance of the connecting wires) is by calculation 0·1 ohm. A little reflection will show that from $\frac{1}{2000}$th to $\frac{1}{3000}$th of the current will be all that can go through the body of the experimentalist. The labourer at Hatfield was killed because a current of high tension was used to overcome the high resistance of the lamps, &c., the body in this case offering a comparatively lower resistance, and forming an easy means of escape for the current. This would have been avoided had there been two leading wires, instead of using the earth in place of one of them. Furthermore, it should be made imperative that all conductors carrying large currents should be well insulated. 2. We are already pledged to an article on "Electrical Measurement."—B. J. P. The best thing you can do is to set to and make a little coil, or a medical magneto machine. You will not have much chance of getting a shock otherwise, unless you are prepared to expend a considerable amount of money on batteries. You might get a little shock by placing the bell in circuit, and so getting a series of pulsations. 2. You appear to be a little in error, or your query is more than I can fathom. Depositing steel by electricity is out of the question.—H. W. B. I should say that such an accumulator might give good results at first, but, from a chemical point of view, would soon run down through local action. I will experiment and publish the result.—F. W. The universal galvanometer has not so far answered expectations, and you cannot do better than get a *good* tangent galvanometer (wound for quantity and intensity with an optional shunt), and a set of Wheatstone bridge coils.—E. KNOWLES. Get an ordinary kite, place a small metal rod on it, and in connection with the iron a wet hemp or cotton thread. At the lower extremity of the thread tie a dry silk thread. Fasten a key or any suitable piece of metal on the lower part of the cotton. Send your kite up, holding it by means of the silk, and sparks can then be obtained from the lower piece of metal. Before, however, you try an experiment on your body, it would be advisable to make arrangements with a respectable undertaker.

THE TELESCOPE.

R. F. S. As your primary object is probably to examine lunar and planetary detail, and to obtain a knowledge of the more remarkable celestial objects, in the shape of double stars and nebulæ, you will certainly obtain more for your money in the shape of a reflecting than of a refracting telescope. A really first-class 3-inch achromatic refracting telescope, mounted on a firm stand, and furnished with two astronomical eye-pieces, is scarcely procurable under £15 or £16; whereas the same sum will purchase a 5¼-inch silvered glass reflector, mounted on a plain equatorial stand (without divided circles), the respective grasp of light of these instruments being nearly in proportion of 9 to 25. It is beyond our province to recommend individual makers, but our Advertising Columns may be consulted with advantage.—JULIUS must not accept every statement in Guillemin's "Heavens" precisely as Gospel. What, however, the writer probably meant to convey (*loc. cit.*) was that, in our climate, 6 inches is as large an aperture as can be profitably employed for ordinary work on average nights. We are occasionally favoured with definition which renders much larger apertures not only available but desirable. Moreover, nothing is easier than to stop down the object-glass of greater diameter on a bad night. But having said this, we must add that the number of *first-class* 6-inch objectives is greatly in excess of that of object-glasses of larger apertures of similar excellence, the difficulty of actually achromatising very large ones being seemingly insuperable. We have ourselves never looked through an objective exceeding 9 inches in diameter which was absolutely and in all respects satisfactory, and we have seen more than one big one which was very indifferent indeed.—H. D. will fail to divide ξ Ursæ Majoris in a 3-inch telescope, unless the object-glass be really a high-class one. But the components of that star are now some 2″ apart, instead of 1″, as our querist appears to imagine; and we must further add that the absolute limit of the dividing power of the finest 3-inch objective in the world is 1·52″. Secondly, an inferior object-glass will not show a trace of Jupiter's satellites on the disc of the giant planet; nor will the best one do so, save just after their ingress on to, or just prior to their egress from, his limb. Thirdly, Argelander's 11·6 magnitude is the *minimum visibile* of the aperture of which we are speaking. Argelander's scale had a scientific basis. Smyth's scale was—well, it was Smyth's scale.—Rev. E. H. can *not* make "a really useful astronomical refractor, object-glass from 3 to 4 inches" for any sum approaching £3 or £4, for the simple but sufficient reason that a high-class 3-inch object-glass alone in its cell costs £6, and a 4-inch £15. Our correspondent should write to the dealers in second-hand instruments who advertise in KNOWLEDGE for their catalogues. Excellent telescopes, by known makers, may often be obtained in this way at reasonable rates.—"Antarctic" may measure the power of any eye-piece on any telescope whatever, reflecting or refracting, by the aid of a little instrument known as the Dynamometer, one very simple form of which is the invention of the Rev. E. L. Berthon, of Romsey, in Hampshire. All he has to do is to focus the telescope on a star, and then, in the daylight, turn it up to the sky. Withdrawing the eye now some ten inches or so from the eye-piece, a little circle or disc of light will be seen. This is an image of the object-glass or mirror diminished in the exact proportion of the magnifying power. All we have to do, then, is to measure the diameter of this spot of light, and to divide the effective aperture of the mirror, or objective, by the result; the quotient will give the magnifying power of that particular eyepiece. Thus, suppose that the aperture of "Antarctic's" mirror is 6½ inches, and the little spot of light is found to measure 0·0252 inch, then, dividing 6·5 by ·0232, we get 280 as the magnifying power. All methods of calculation based on obtaining the foci of the component lenses of an eye-piece are very operose, and uncertain to boot.

XUM

Our Mathematical Column.

FAIR BUT UNWISE BETTING.

By the Editor.

BEFORE considering other orders of chance problems, it will be well to consider the relation between the mathematical chance of an event and the moral value of expectations depending upon it. For convenience, let us do this with special reference to wagers upon events more or less probable, such as races, matches, and so forth.

If the chance of an event is $\frac{r}{n}$, the chance of its failing to happen is $\frac{n-r}{n}$. Comparing these two chances, we get the ratio r to $n-r$, in which r represents the number of favourable cases, and $n-r$ the number of unfavourable cases. The technical expression used to indicate this relation is that the odds are r to $n-r$ on the event (that is, in favour of it), if r is greater than $n-r$; or $n-r$ to r against the event, if r is less than $n-r$.

Suppose now that in an urn there are ten balls, of which three are white and seven black; then the chance of drawing a white ball is $\frac{3}{10}$, and the chance of failing to draw a white ball is $\frac{7}{10}$; while the odds against drawing a white ball are 7 to 3. And if two persons, A and B, were to wager on the event, A to win if a white ball were drawn, and B to win if a black ball were drawn, then, that the wager should be strictly fair, the sums respectively wagered by A and B should be in the proportion of 3 to 7. It will be clear that this proportion is fair, if we remember the real fact as respects wagers, that when once a wager has been laid, even though the betters keep the wagered sums in their pockets till the issue is decided, the case is precisely the same as though those sums were added together to form the *prize* for the winner. In the present case, supposing A to wager £3 against £7 of B's, the prize for the winner is £10; and as A's chance is $\frac{3}{10}$, the price he should pay for it is three-tenths of £10—that is, £3—while B's price for his chance should be seven-tenths of the prize, or £7.

But there is another way of viewing the matter. Suppose A and B to go on betting upon the same event, A always backing the white and B the black, the drawn ball being returned after each wager had been decided; then, in the long run, the number of times that A and B would be respectively successful would be in the proportion of 3 to 7, as nearly as possible—the more nearly the longer the backing continued; and it is clear that, to equalise their chances, the money gained by A and B respectively, when successful, must be in the proportion of 7 to 3.

Here, then, we have the mathematical principle on which all wagers should be based, if they are to be fair,—viz., that the sums respectively staked by the bettors must be proportioned to their respective chances of success.

But although bets made on this principle are strictly fair as between the parties to the wager, yet it is a mistake to conclude that a man's chances of loss or of gain are equal, when he stakes his money on fair wagers.

For, in the first place, his property is not increased in the same proportion if he wins an even wager, as it is diminished if he loses. Thus, suppose his property to be £1,000, and that he wagers £500 against £500, the chances of success and failure being equal. If he loses, his property is halved; but it is not doubled if he wins; and in like manner it may be shown that, whatever he stakes, the effect of success is not equivalent to the effect of failure.

It might seem, however, that if a person always wagered a sum bearing a very small proportion to the property he has at first, he would be safe from serious loss in the long run. Supposing, for example, that a person, A, has £1,000, and repeatedly wagers £1 against £1 on equal terms, it might seem as though he would never be much richer or much poorer than at starting. Now, even if this were so, it would be an argument against betting, since it would show the uselessness of fair wagering. But, as a matter of fact, a belief in the "long run" is one of the most fatal delusions which a bettor can entertain. It may be shown—and, indeed, will be found to follow from the principles to be enunciated in these papers—that the chance of absolute ruin, in such a case as we have imagined, increases with the number of wagers. The *ratio* of money lost to money won in such a series of wagers approaches, indeed, more and more nearly to equality the greater the number of wagers; but the extent of the *difference* between the two sums is likely to be greater the longer the process of wagering is continued. Thus, in a hundred wagers there would be nothing very wonderful if A lost or won as many as fifty-five wagers, in which case he would have lost or won £10; whereas in a million wagers it would be utterly improbable that he would lose or win so many as 550,000 wagers; the numbers of won and lost wagers would probably be much closer; but it would be unlikely that they would be so close as 500,500 and 499,500; yet *if they were no closer, and the balance were against* A, his £1,000 would be lost, and his wagering put an end to. It is calculable that the odds are greatly in favour of the numbers not being so close as 500,500, and 499,500, and it is obvious that the balance is as likely to be against A as in his favour. So that what he in effect would risk by entering on so long a series of wagers would be this, that *in all probability* his whole property would be as if risked on a single contingency, in which the chance of success or failure was but one-half. No one would think of risking his whole fortune on the toss of a halfpenny; nor would any one care to agree that his whole fortune should be thus risked, *if* in drawing a ball out of a bag of twenty, of which but one was white, he failed to draw the white ball. Yet a person who makes a series of small wagers, trusting to the "long run," is no whit better circumstanced (if he only continues wagering long enough) than one who has agreed to so daring a venture as the latter; while the longer his wagering is to last, the more nearly does his case approach that of the former. For the complete investigation of the subject of wagering, I would refer the reader to the chapter on the "Risks of Loss and Gain" in De Morgan's admirable, though somewhat dry, treatise on probabilities; but the following general principles may be enunciated, as containing the essence of the whole matter:—Better small wagers and many than large wagers and few; better few small wagers than many small wagers; better yet, no wagers at all.

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[30]—T. F. asks for the solution of equations—

$$x^2+y=11 \quad \text{(i)}$$
$$y^2+x=7 \quad \text{(ii)}$$

The equations, of course, reduce to a biquadratic in x or y, having one obvious root, and so reducing to a cubic, the solution of which does not belong to elementary algebra. The four roots are all real, as is indeed obvious if we consider that (i) and (ii) are equations to two parabolas having their axes at right angles, and intersecting in four points. In the only sense in which the equations can be regarded as suitable for our "young readers" as we wrote, their solution is very easy, because $x=3$ and $y=2$ are obvious solutions, so that $x-3$ will be a factor of the biquadratic in x, or $y-2$ a factor of the biquadratic in y, according to the line followed in obtaining an equation with one unknown. Or we may write (i) and (ii) thus—

$$x^2+y=3^2+2 \text{ and } y^2+x=2^2+3$$

whence obviously $x=3$ and $y=2$.

The Muscular Force of a Crocodile's Jaw.—A strange kind of experiment has been lately made in Paris by Drs. Regnard and Blanchard, viz., the measurement of the power exerted by the masseter muscle in a crocodile (a muscle passing from the cheek-bone to the lower jaw). Ten live crocodiles of the species *C. galeatus* or *siamensis*, that had been sent in large cases from Saigon to M. Paul Bert, afforded the opportunity for such experiments. Some of these animals were as much as 10 ft. in length, and weighed about 154 lb. The difficulty of managing such creatures in the laboratory was, of course, considerable. The crocodile was fixed with ropes on a heavy table; the lower jaw kept in contact with the table by a cord, while the upper was raised by means of a cord attached at the extremity, and passing up to a beam overhead. A dynamometer was inserted in this cord, and was affected when the animal was stimulated with an electric current. In this way a crocodile of about 120 lb. weight gave an indication of about 308 lb. (140 kilogrammes). The application of the cord at the end of the snout was necessary, but unfavourable, seeing the application of the force is thus at the end of a long lever, and there is at least five times more space between this point and the insertion of the masseter muscle than between the latter and the joint of the jaw, the fulcrum. Hence the masseter really produces a force five times that indicated by the dynamometer, or about 1,540 lb. (700 kilogrammes). This extraordinary force, it should be remembered, was that of an animal somewhat weakened and at a low temperature. The force (of about 308 lb.) is really applied at the end of four large teeth that project beyond all the others, and considering the surface here represented, the authors estimate the pressure, while the bite is executed by the extremity of those teeth, at nearly 400 atmospheres. Making similar experiments with an ordinary sporting dog, they obtained in the dynamometer a pressure of about 72 lb.; while the effect at the insertion of the masseter was about 360 lb. The pressure at the point of the canine teeth would be about 100 atmospheres. It is calculated that the crocodile is about one-third stronger than a dog of the same weight would be.—*Times*.

XUM

Our Whist Column.

BY "FIVE OF CLUBS."

PLAY SECOND HAND (TRUMPS).

THE play second hand in trumps differs in several respects from the play in plain suits. This partly depends on the circumstance that the lead in trumps is somewhat different, as we have seen, from the lead in the other suits; partly on the absence of risk from ruffing; and partly on the card turned up, and its position with respect to second player. Then, also, the critical nature of trump play has to be considered. When trumps are led, second player knows that there is at least sufficient strength, either in the leader's hand, or between the leader and his partner (if the latter has signalled), to justify the expectation that between them they may get out all the trumps held between second and fourth players, and bring in a long suit. A defensive, or at any rate a waiting game, has therefore generally to be played.

The principal differences in detail, between trump and plain suit play, second hand, are these:—

From Ace, King, and one or more small cards in trumps, it is generally better to play a small card second hand, to give partner a chance of making first trick. A small card is played for a similar reason in trumps, from King, Queen, and more than one small one; if, however, you have the ten also, play it. From Ace, King, Queen, and a small one, Queen is played in Trumps, as well as in plain suits, because second player is so strong that he should play a forward game.

From Ace, Queen, ten, in trumps, ten is played, instead of Queen as in plain suits. This gives partner a chance of making the trick; and should he fail and third hand make it, on the return of the trump lead two tricks are certain.

From Ace, Knave, ten, one or more small ones, the smallest is played in plain suits, because the first player cannot hold both King and Queen. But in trumps (see Leads) he may hold both these cards, and it is therefore better to play the ten.

From a honour and one small card, only play the honour if it is important to stop the trump lead. If your partner has turned up King or Ace, and you hold Queen and a small one, you of course play the small one.

SECOND HAND SECOND ROUND.

In the second round of a suit it is nearly always best, if you hold the winning card, to play it, unless, of course, you know third hand to be very weak in the suit. When your adversaries' trumps are exhausted it is sometimes better to pass the second round of a long suit, if the third round is assured, and there is a fair chance of the suit being established third round. In trumps second round, it is often better to keep back the winning card if you have numerical strength in trumps and a good plain suit.

If in second round second hand holds second and third best cards, he should, of course, play the third best. If you hold third best, and have reason to believe your partner holds the best (and leader second best) you may often with advantage play the third best, and so save your partner's best. If, however, you are long in the suit, you very likely lose by this, for your partner's best card is probably single, and so falls on a trick already won.

ILLUSTRATIVE GAME.

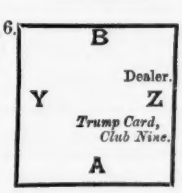
Ace is usually played second hand on King; but occasionally, with Ace, Knave, and others, if you are strong in trumps you may pass the King, on the chance of making the tenace. It is, however, very seldom good play to do this.

The following game illustrates the importance of clearing your partner's suit when it is established, and you hold the best card and but one small one. With two small ones, *B*, second round, would not have been justified in taking his partner's trick.

THE HANDS.

A.
Clubs—K, 3.
Hearts—A, Q, Kn, 9, 8, 6.
Spades—Q, 7, 3.
Diamonds—5, 2.

Y.
Clubs—Q, Kn, 7, 2.
Hearts—4, 3.
Spades—A, K, Kn, 6, 5.
Diamonds—A, K.



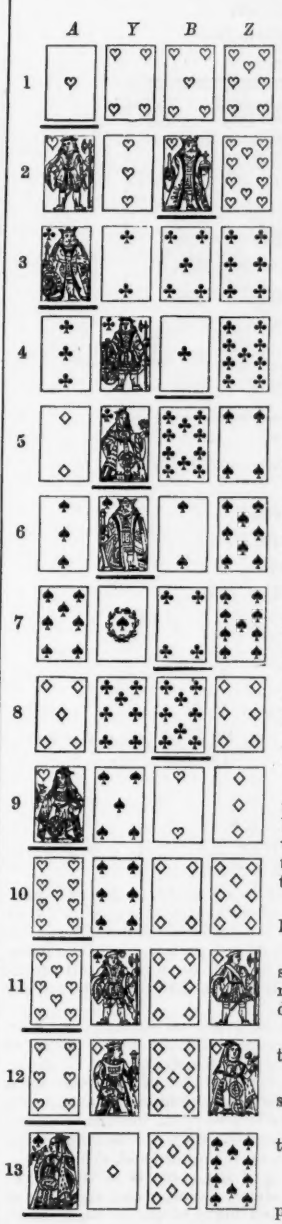
B.
Clubs—A, 10, 8, 5, 4.
Hearts—K, 5, 2.
Spades—2.
Diamonds—10, 9, 7, 4.

Z.
Clubs—9, 6.
Hearts—10, 7.
Spades—10, 9, 8, 4.
Diamonds—Q, Kn, 8, 6, 3.

Score.—*A B*, 0; *Y Z*, 0.

THE PLAY.

NOTE.—The underlined card wins trick, and card below it leads next round.



REMARKS AND INFERENCES.

1. Both *Y* and *B* begin to signal, both with good reason, but *Y* with the better, for he not only has four trumps two honours, but an excellent hand outside trumps.

2. *A* has led from Ace, Queen, Knave, and two at least. *Y*'s signal is completed; but *B* sees his opportunity, takes the trick, (leaving his partner with the suit established), and leads the penultimate trump. A bad partner would have saved his King (knowing the Queen with *A*), and lost the chance of making a great game.

4. *B* having five trumps, and knowing *Y* cannot have less than four trumps two honours (*Y* being one who never signals without good cause), knows that *A*'s Club 3 cannot be the lowest of three left in hand; but it is the lowest Club; therefore *A* can have no more. For if he had had two he would have played the higher. The inference happens to be of no subsequent use to *A*, but it is well to note it, as inexperienced players often lose by failing to notice just such points as these. *Y* makes a similar inference, being sure that *B* would not have led trumps, after opponent's signal, from less than five trumps, one honour. *Y* should have played the seven, on the chance that ten may lie with *Z*. As the cards lie, it would have made no difference in the result.

5. Having second and third best trumps left, *B* leads the second best and draws *Y*'s Queen. *Y* does well to take the trick, having nothing to gain from getting out more trumps.

6. *Y*, of course, resorts to his long suit; but unfortunately,

7. *B* has but one card of the suit, and trumping the second round (*Z* discards penultimate diamond),

8. Draws out the last of *Y*'s trumps,

9. Brings in his partner's long suit, and

10, 11, 12, 13, *A B* make five by tricks.

W. F.—Would it not be apt to perplex if Queen with two small cards, or Knave with three small cards, were said to be singly guarded? Of course, Queen with two guards is not much better guarded than King with one. (She is a little better guarded, because of the chance that if in first round the trick falls to a small card, second round may draw Ace and King together.) But that is met by keeping the expression "Queen guarded," for Queen two small cards, "Knave guarded" for Knave, three cards, and so on. A single card is a guard, though an imperfect one, for a Queen.

H. W.—No; certainly *A* cannot take back the card first played, unless it would have been a revoke. Not only so, but the card which he threw down to replace the first can be called. If a player having renounced finds, before the trick is turned and quitted, that he has a card of the suit, he can take up the card played in error, and follow suit; but the card so played may be called.

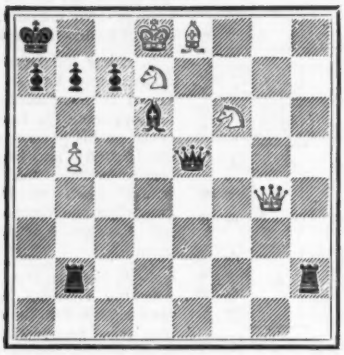
XUM

Our Chess Column.

PROBLEM No. 39.

By the late William Bone.

BLACK.



WHITE.

White to play and mate in four moves.

SOLUTIONS.

PROBLEM 35, by Leonard P. Rees, p. 525.

1. Q to QR8, and mate next move.

PROBLEM 36, p. 525.

1. R. to B8 (ch)	1. K to Kt 2
If K takes R, Q to K8 (mate)	
2. Q to Kt6 (ch)	2. B takes Q
3. P takes B (ch)	3. K takes P.
4. B to Q3 (mate)	

PROBLEM 37.

1, Q takes P (ch); K to R sq.	2. Kt to B7 (ch); K to Kt sq.
3. Kt takes RP (ch); K to R sq.	4. Q to Kt8 (ch); R takes Q.
5. Kt to B7 (mate).	

REVIEWS.

Chess and Mathematics. (From "Waifs and Strays.") BY CAPTAIN HUGH A. KENNEDY.

Tom Moore, the poet, mentions in his diary, as a very strong objection of Bishop Warburton's to mathematical pursuits, "that in making a man conversant with studies in which certainty is the result, they unfit him (or at least do not prepare him) for sifting, and balancing (what alone he will have to do in the world) probabilities; there being no worse practical men than those who require more evidence than is necessary."

Now that circumstance, which so grave an authority as Warburton pronounces to be an inherent deficit in mathematics, as regards practical mental training, is precisely reversed in Chess, and constitutes, therefore, the principal value of the game as a mental exercise and preparative for the contentions of actual life. In the study of mathematics, there is always an inevitable result, to be reached by a fixed and consecutive train of reasoning which admits of no deviation. In Chess, on the other hand, although the result, i.e., the winning or drawing of the game, is inevitable, yet the ways of accomplishing it are almost infinite; and the mode of reasoning—save in the openings and many of the endings, which can be acquired from the books—consists in a perpetual sifting of probabilities. It is true that Chess in its nature admits of the same determinate certainty as mathematics, but as it is also true that absolute perfection of play is possible only by the exercise of a degree of prescience and a depth of skill, both in combination and calculation, of which the most happily-organised brain must ever fall immeasurably short, this perfection, as far as concerns us, cannot be said to have any existence.

Chess Trees. By THOMAS LONG, B.A., T.C.D. Tree I.—Philidor's Defence.

The whole of this opening is on one foolscap folio, in what the author calls "map and pedigree" form. In our opinion, the form adopted by Cook in his "Synopsis" is much plainer and simpler, and we can see nothing in Mr. Long's plan but unnecessary elaboration. On examining the sheet we found it to contain exactly the same matter and variations as is given in the "Synopsis," with hardly anything novel. There are a few transpositions of moves or slight additions, which do not, however, materially affect the opening. There are twenty-two variations, and we will trace them to Cook's, giving the number of the variations in that book.

"Chess Trees" variation ...	AB	C	D	EF	H	I	K
Cook's "Synopsis" variation	17.	7.	8.	5.	6.	4.	3.
"Chess Trees" variation ...	I.	M.	N.	O.	PQ.	R.	
Cook's "Synopsis" variation	2.	1.	12.	11.	10.	9.	
"Chess Trees" variation ...	S	TU.		W.	X.		
Cook's "Synopsis" variation	18.	16.		13.	15.		

Seeing, then, that the form in which this opening is given is less clear than Cook's, and the contents the same, we turn to the question of price; the book, when finished, will cost about seven or eight times the price of the "Synopsis." In the latter book there are thirty-four different openings, with over 600 variations, for which a charge of 3s. 6d. is made. Mr. Long asks the chess-public to give 1s. for one opening containing twenty-two variations.

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Muzio solution of Nos. 35 and 36 correct; in the Evans' play 8. P to Q4, your best move, or, better still, 6. P to Q3.

H. S. S.—Solutions of 33 and 34 correct.

M. Oldwyd.—No. 35 correctly solved.

Steady Reader.—Your former solution no doubt correct, but we did not receive it.

H. A. N.—Problem received with thanks.

Alfred B. Palmer.—No. 38 correctly solved, and in good time.

S. M.—You are right in a measure; Black would not lose the piece if he did not proceed with B to B4, but if he did not intend to push the attack, why institute it at all? after 15. B takes B. 16. Q takes B; 16. Kt to R3. 17. P to KB4. White has clearly a better game.

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